Psychology and Scientific Research.
I. The Nature of Scientific Inquiry

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The traditional code of science—that is, the objectives sought and the methods of investigation—cannot satisfy the requirements of our critical times, and this is why science has failed to measure up to the opportunities and obligations before it. The generally accepted ideas of what natural science is and what it is for are out of date and need radical revision.—C. J. Herrick (*8*, V)

A FEELING OF URGENCY for a more adequate understanding of man and his social relations can be sensed in today's intellectual atmosphere. People are becoming more and more anxious about the ability of psychologists and social scientists to help solve the problems arising from our technological advances and from the swift social transitions they leave in their wake. But unfortunately what Herrick has said about the natural sciences applies especially to those sciences which deal with man—psychology and the social sciences in general. Moreover, in these sciences, in contrast to the physical sciences, there seems to be less agreement as to what constitutes significant research.

Obviously, an increase in our understanding of man can come about only as we extend our empirical knowledge and improve our formulations through research of demonstrated significance. And before that is possible, we must increase our understanding of the scientific process through which discoveries are made. But sometimes the scientist's interest in building up the content of his discipline sidetracks him from a consideration of the scientific process itself and creates a lag in the understanding and improvement of scientific tools. What follows is an attempt to clarify our thinking about the nature of scientific research in those fields which take upon themselves the primary responsibility of accounting for man's thoughts and behavior. Only then will such research accomplish what we have a right to expect of it.

We shall first consider the nature of scientific inquiry, trying to find out why man pursues scientific inquiry, anyway—what function it serves him, and what steps seem to be involved. We shall then distinguish between scientific inquiry and scientific method—a distinction which seems necessary to avoid certain pitfalls and to assure scientific progress. Then we shall try to point out some of the specific implications to be derived for psychology from a better understanding of the nature of scientific inquiry and the role of scientific method and we shall indicate to what degree science can be "objective." Finally, some suggestions will be made which might accelerate the kind of scientific research that will increase our understanding of man.

The apparent reason for scientific inquiry is essentially the reason for any inquiry—to solve a problem. Scientific inquiry can never be understood if it is somehow put on a pedestal and viewed as something remote and apart from man's everyday activities. "Science," says Conant, "emerges from the other progressive activities of man to the extent that new concepts arise from experiments and observations" (*1*, 24).

These activities of life are carried through in an environment which includes people, artifacts, the phenomena of nature. Man's only contact with this environment is through his senses. And the impressions man's senses give him are cryptograms in the sense that they have no meaning unless and until they become functionally related to man's purposive activities. The world man creates for himself through what Einstein has called the "rabble of the senses" is one that takes on a degree of order, system, and meaning as man builds up through tested experience a pattern of assumptions and expectancies on which he can base action.

Man builds up his assumptive or form world largely in an unconscious and nonintellectual way, in the process of adjustment and development as he goes about the business of life, that is, as he tries to act effectively to achieve his purposes. Man often uses many of his assumptions without being at all aware of them, such as those involved in reflex activity, habits, stereotypes, and a whole host of perceptual activities. Man is aware of other assumptions from time to time as they become relevant to the situation at hand, such as loyalties, expectancies, ideals. Still

others, such as intellectual abstractions, can be brought to voluntary recall. Man's actions cannot be effective unless and until he builds up an assumptive or form world that has some degree of constancy and verifiability.[1]

What man brings to any concrete event is, then, an accumulation of assumptions, of awarenesses, and of knowledge concerning the relatively determined aspects of his environment as derived from his past experiences. But since the environment through which man carries out his life transactions is constantly changing, any person is constantly running into hitches and trying to do away with them. The assumptive world a person brings to the "now" of a concrete situation cannot disclose to him the undetermined significances continually emerging. And so we run into hitches in everyday life because of our inadequate understanding of the conditions giving rise to a phenomenon, and our ability to act effectively for a purpose becomes inadequate.

When we try to grasp this inadequacy intellectually and get at the why of the ineffectiveness of our purposeful action, we are adopting the attitude of scientific inquiry. Man as scientist tries to understand what aspect of his environment is responsible for a hitch and then calls upon what knowledge he has that is relevant to an understanding of the determined, predictable nature of the particular phenomenon in question. Modern man uses the scientific method as a tool because he has found empirically that he can increase his understanding and act more effectively if his pursuits are guided by some knowledge concerning the determined aspects of the phenomenal world. G. H. Mead pointed out (*9*, 41) that

> . . . Every discovery as such begins with experiences which have to be stated in terms of the biography of the discoverer. The man can note exceptions and implications which other people do not see and can only record them in terms of his own experience. He puts them in that form in order that other persons may get a like experience, and then he undertakes to find out what the explanation of these strange facts is.

Since the scientist's acquired purpose is to increase his understanding of a certain range of phenomena, when he experiences a hitch in his understanding of such phenomena he tries to bring to conscious awareness the reason for the hitch, that is, he tries to formulate intellectual concepts that will explain away the hitch. He does this by examining the probable conditional relationships except for which he, as an experiencing individual in a concrete situation, would not be faced with the hitch. He abstracts out of the hitch-situation those aspects he believes are probably necessary to his understanding of the original hitch. In his inquiry, the scientist arbitrarily treats these abstracted aspects of a phenomenon as if they existed in their own right. He does not do this simply because he wants to but because he has to, in order to recall and manipulate the phenomenon intellectually. The abstractions man is able to form have on him what Dewey and Bentley characterize as a tremendous "liberative effect," making possible the voluntary controlled conceptual thinking necessary for scientific inquiry and for the use of scientific method.

From this point of view, we might say in general that science is an activity designed by man to increase the reliability and verifiability of his assumptive world. For it would appear that in the last analysis any scientific pursuit—no matter how abstruse it seems—is carried on because it is somehow of concern to man. Science is the human effort to understand more about nature and human nature in verifiable, determined terms. The word *determined* is used here in the scientific sense as meaning high in prognostic reliability. From this it is clear that real progress in any science involves an awareness of our assumptive worlds, a consciousness of their inadequacy, and a constant, self-conscious attempt to change them so that the intellectual abstractions they contain will achieve increasing breadth and usefulness. Real progress in science means much more than merely adding to existing knowledge.

The processes involved in scientific inquiry would seem to be somewhat as follows: (1) sensing the inadequacy of the conceptual aspects of our assumptive world, thereby being faced with a problem for which we must seek an answer; (2) deciding on all those aspects of a phenomenon that might have a significant bearing on the problem: deciding on those aspects except for which the functional activities in question would not exist; (3) picking out from the various aspects assumed to be involved those that seem most important in terms of the original hitch we faced and that will serve as bases for standards we can think about and manipulate; (4) working out some method of changing those aspects we have chosen as variables or bases for standards and conducting our empirical investigations accordingly; (5) modifying our assumptive world on the basis of the empirical evidence concerning the validity of formulations that have resolved an immediate problem.

The solving of the immediate problem will automatically give rise to new hitches and the above process constantly repeats itself.[2]

[1] The nature and function of man's assumptive or form world is much too large a subject to treat in any detail here and must be reserved for later consideration.

[2] There seems to be a striking similarity between the processes used in scientific inquiry and the processes man makes use of in building up the assumptive world. Both science and common sense can be regarded as functional activities man uses in carrying out his life transactions. And

Specifically, it seems that scientific inquiry has two major functions for man. First, it provides man with a bundle of what are called "scientific facts." This bundle is composed of his up-to-the-now understandings of the determined, predictable aspects of nature and is used by him for purposes of prediction and control. There are essentially two varieties of these scientific facts: general statements of relationships of determined aspects of nature which we refer to as "scientific laws" and which, in the physical sciences, tend to be expressed in mathematical formulas; second, applications of these general laws to concrete situations for purposes of verification, specific prediction, or control. The characteristic of all these generalized scientific laws is that they disclose predictable aspects of types of phenomena no matter where or when they occur, irrespective of actual concrete situations.

A second function of science is that it provides a conceptual reorganization of the knowledge man has already acquired of the determined aspects of nature. Here we are trying to increase our range of understanding, or, as Dewey and Bentley phrase it, to improve our "specification," that is, our accuracy in naming (*4, 5*). Here, for example, the specifications involved in relativity are more accurate namings of phenomena than are Newton's concepts, and in this sense, Newton's concepts are not to be regarded as "wrong." This function of science includes that of increasing the range of man's conceptual knowledge through the discovery of more and more predictable aspects of nature that up to the present time remain undetermined.

Understanding and prediction. The aim of science is often defined as the attempt to increase the accuracy of our predictions. While the accuracy of predictions is clearly a most important criterion of progress in scientific formulation, emphasis on prediction alone can easily obscure the more fundamental aim of science covered by the word *understanding*. When we use the word understanding we are giving emphasis to the importance of increasing the range of our conceptual knowledge. Increased accuracy of prediction will be an inevitable coproduct of increased understanding in this sense. Any increase in understanding is also inevitably accompanied, sooner or later, by an increased ability to control variables and to apply our knowledge. Understanding also avoids the implication of a rigid determinism which seems, among other things, to be inconsistent with the fundamental indeterminism of modern physics.

Every scientific investigator must bear in mind that it is impossible for scientific research to disclose the unique specificity involved in any one actual occasion—e.g., the student of modern physics knows that there is no law governing the behavior of an individual atom. And the investigator must also remember that it is impossible to predict with any complete accuracy the specific nature of growth and emergence, which are themselves undetermined. While it is impossible to determine the undetermined nature of emergence, it is still possible to increase our scientific knowledge about emergence through understanding more about the relatively determined phenomena immediately related to these undetermined emergent aspects. For example, we may hope to understand more about the extent of the undetermined field; to understand more about the conditions which make it possible for the undetermined aspects to emerge. In other words, our understanding of emergence can improve only insofar as we become more and more aware of the boundaries of our determined world.

It is here that many of those who equate science with prediction or who use a narrow working definition of operationism are also those who will say they want nothing to do with the speculations of philosophy. And yet it is only by taking the philosopher's point of view, by bringing in freely all factors that might conceivably be involved in a single situation, that we can become aware of the boundaries of our up-to-the-now, determined scientific world. In discussing the role of philosophy, Conant writes that "there must be constant critical appraisal of the progress of science and in particular of scientific concepts and operation" (*1,* 13 f.). In their book on *The evolution of physics,* Einstein and Infeld repeatedly emphasize the new philosophic views which have both helped to evolve and have evolved from physical research. Any scientific investigator who pushes his

method of scientific inquiry seems in many ways to be an unconscious imitation of those age-old processes man has employed in his common-sense solutions of problems. In common-sense activity, the assumptions and awarenesses on which man depends for effective action are the hypotheses he has built up from his many experiences: weighted averages he unconsciously uses to give him a high prognosis for effective action.

There are, however, certain important differences between the steps involved in pursuing scientific inquiry and the apparent processes that constitute common sense. A most important difference is the fact that in using scientific inquiry, man is the operator who decides what he is going to operate on and how. In an everyday life situation, however, man is not only the operator but he is also being operated on and must carry out his activities in the midst of the situation itself. When we meet hitches in everyday life and try to overcome them with hunches for effective action, we test these hunches by the action itself in a more or less insightful, more or less conscious way. In scientific inquiry, on the other hand, hunches are tested by controlled experiments and a deliberate attempt is made to intellectualize the processes involved (cf. *3*).

field of inquiry beyond the realm of the determinable and the repeatable out into the no man's land of emergence will inevitably become entangled with metaphysical problems. In so doing, he can hope that what is metaphysical for him today can tomorrow be part of the understood, physically determined, repeatable, and verifiable.

Transactional observation. Our own philosophical basis for our thinking concerning the nature and function of scientific inquiry and scientific method should be made explicit. We are using as our take-off point what Dewey and Bentley have referred to in a series of articles as a "trans-actional approach."[3] What they mean by the term *transactional* can best be gathered by their own words. "Observation of this general (transactional) type sees man-in-action not as something radically set over against an environing world, nor yet as merely action 'in' a world, but as action *of* and *by* the world in which the man belongs as an integral constituent" (*4,* 228). Under this procedure all of man's behavings "including his most advanced knowings," are treated as "activities not of himself alone, nor even as primarily his, but as processes of the full situation of organism-environment" (*6,* 506). "From birth to death every human being is a *Party,* so that neither he nor anything done or suffered can possibly be understood when it is separated from the fact of participation in an extensive body of transactions—to which a given human being may contribute and which he modifies, but only in virtue of being a partaker in them" (*3,* 198).

Dewey and Bentley distinguish this transactional procedure from two other procedures which they feel have largely dominated the history of science up till now. First is what they call the antique view of "self-action; where things are viewed as acting under their own powers." Second is the interaction view of classical mechanics, "where thing is balanced against thing in causal interconnection." In transactional observation, "systems of description and naming are employed to deal with aspects and phases of action, without final attribution to 'elements' or other presumptively detachable or independent 'entities,' 'essences,' or 'realities,' and without isolation of presumptively detachable 'relations' from such detachable 'elements'" (*6,* 509).[4]

While it is easy enough to understand this point of view intellectually, it is not nearly so easy to put it into operation in pursuing actual scientific inquiry. It tends to go against the grain of the psychologist's working procedures to regard any formulation merely as a certain "connection of conditions" (*2,* 217). And it is perhaps particularly difficult for psychologists to understand the full implications of the transactional point of view, because, as Dewey and Bentley have pointed out, "The interactional treatment, as everyone is aware, entered psychological inquiry just about the time it was being removed from basic position by the physical sciences from which it was copied" (*7,* 546). But we must remember that psychology, by comparison, is still in its infancy, that the transactional approach, which Dewey and Bentley trace to the preface of Clerk Maxwell's *Matter and motion,* dated 1877, antedated the first psychological laboratory.

[3] Since this article was written, Dewey and Bentley have brought together in a single volume, *Knowing and the known* (Boston: Beacon Press, 1949) references *3, 4, 5, 6,* and 7 cited here together with other articles previously published by them.

[4] In citing these distinctions made by Dewey and Bentley we are not implying (and they may not be) that in our own view either self-action or interaction can by any means be completely ruled out in any adequate explanation. Self-action is seen in the behavior of the simplest bodily cell, in the uniqueness of individual behavior, in the behavior of "nations," etc., while interactional assumptions appear to be essential first steps in providing an intellectual grasp of the form for the flow of transactional processes. The role of self-action and interaction in an inclusive transactional view must be left open as a problem, and cannot be considered here in detail.

(This is the first of a series of three articles.)

References

1. CONANT, J. B. *On understanding science.* New Haven: Yale Univ. Press, 1947.
2. DEWEY, J. *Problems of men.* New York: Philosophical Library, 1946.
3. ———. *J. Phil.,* 1948, **45**, 197.
4. DEWEY, J. and BENTLEY, A. F. *J. Phil.,* 1945, **42**, 225.
5. *Ibid.,* 1946, **43**, 645.
6. *Ibid.,* 505.
7. *Ibid.,* 535.
8. HERRICK, C. J. *George Ellett Coghill.* Chicago: Univ. Chicago Press, 1949.
9. MEAD, G. H. *Mind, self and society.* Chicago: Univ. Chicago Press, 1939.

The MIT International Conference on the Physics of Very Low Temperatures, September 6–10, 1949

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LOW TEMPERATURE PHYSICS is a field which has been slow and deliberate in its growth. During the last century a group of brilliant experimenters succeeded in liquefying one after another of the "permanent" gases, until finally helium succumbed, and there were no more left to conquer. This early work was international: England, Holland, France, a number of countries took part in the progress. Then during the first years of the present century a great laboratory devoted entirely to very low temperatures was set up at Leiden, by Kamerlingh Onnes. A succession of brilliant discoveries came from this great installation, and established it as the foremost laboratory of the world in the field. It was not long to stand unchallenged, however. In England, Kapitza pioneered in the modern school of low temperature work, at Cambridge; there the Royal Society Mond Laboratory was founded, carrying on in the field after Kapitza's departure for Russia, and today, is one of the leading English low temperature laboratories. The Clarendon Laboratory at Oxford, profiting from the departure from Germany of some of the leading low temperature workers of that country as a result of Hitler's policies, has built up a low temperature laboratory of the first magnitude. The Russian school of low temperature physics, established by Kapitza on his return to that country, forms one of the leading groups in the field. In Canada the laboratory at Toronto, in the United States those at the University of California and the National Bureau of Standards, were working before the war. A few other institutions have been active, but before the war it was a branch of physics confined to a few places, largely because of the magnitude of the installations required, and the difficulty of the experimental techniques encountered.

Since the war, the interest in low temperature physics has greatly increased, particularly in the United States. There are probably two quite different reasons for this. One is a practical, experimental one. The development of new techniques of liquefaction, as a result of work undertaken for military purposes, has made the production of liquid helium (at least in this country, where the helium itself is plentiful) no longer a major undertaking, but a relatively easy task. This has led a considerable number of laboratories to move into the fascinating field of low temperature research, perhaps partly unaware of the fact that the production of liquid helium is only a small part of the experimental problem encountered in getting significant new scientific results. The other reason for the increasing interest in very low temperatures is probably the attraction of a field where major problems still stubbornly refuse to yield to theoretical explanation.

There are probably three principal lines of low temperature research at present: superconductivity, the properties of liquid helium, and low temperature paramagnetism, with its related technique of adiabatic demagnetization. The first two of these still defy all attempts at the construction of theories. It is true that partial theories exist; but they are very far from being complete or satisfactory. This is the only point at which the study of matter in bulk still shows the qualities of a challenging puzzle, qualities which are so characteristic of nuclear physics. Until the physicist can explain these strange facts, he will not rest content.

The other active field, low temperature paramagnetism and adiabatic demagnetization, shows no such mysteries, but it nevertheless has its particular reasons for being interesting just now. It relates to the energy levels of electronic spins in crystals. These energy levels are associated with absorption frequencies in the microwave part of the spectrum; and the greatly expanded interest in microwaves as a result of wartime developments has turned physicists in this direction. Not only the electronic, but also the nuclear resonance is concerned in these properties; very shortly after the war, it was found that nuclear absorptions occur in the radiofrequency part of the spectrum. These studies lead to an exciting possibility: just as the adiabatic demagnetization using electron spins has led to the lowest temperatures so far attained, a few thousandths of a degree, so there is the possibility that use of nuclear resonance may make it possible to reach temperatures of the order of millionths of a degree.

Before the war, there were relatively few low temperature physicists. They had meetings, among others, every four years in connection with the Refrigeration Congresses of the International Institute

of Refrigeration, an organization largely devoted to the practical side of refrigeration, but having a very active committee, largely stimulated by the group at Leiden, concerned with the fundamental side of very low temperature research. This organization became dormant during the war, but in July 1948, at the Amsterdam meeting of the International Union of Pure and Applied Physics, a group of low temperature workers was called together, at the initiative of Gorter of Leiden, to consider reviving the international meetings of low temperature physics. Two suggestions were made: either to encourage the International Institute of Refrigeration to resume its activities, or to organize a Commission on the Physics of Very Low Temperatures under the auspices of the International Union of Pure and Applied Physics, an organization which has been much more active since the war than before. Even better than either of these suggestions was the possibility of following both of them. This is essentially what is being done.

The International Union of Pure and Applied Physics was already in active existence, it was known to be favorable toward a suggestion for forming a low temperature commission, and Gorter and Slater, both of whom were at the Amsterdam meeting, were vice presidents. Accordingly it was felt that a meeting could be arranged more expeditiously through this union than through the International Institute of Refrigeration. The suggestion was made that a conference be called for 1949, associated with the Union of Physics, preferably in America, and an invitation was at once issued by the Massachusetts Institute of Technology. It is this conference which has just occurred. On the other hand, the other approach was not neglected. The scientific committee of the International Institute of Refrigeration has been reestablished, under the joint presidency of F. E. Simon of Oxford and C. J. Gorter of Leiden, and it is actively working on plans for a future conference on the physics of very low temperatures, under the joint auspices of the Institute of Refrigeration and the Union of Physics, to meet probably in England in 1951. Meanwhile, the low temperature commission of the Union of Physics has been constituted, under the presidency of Simon, with Gorter acting as secretary, and it held its first meetings in connection with the MIT conference.

The planning of the MIT conference was carried out by Simon, Gorter, and Slater, with much use of the airmail, as well as personal discussions on the occasion of the Amsterdam meeting, and at more recent opportunities. It was realized from the first that, in order to have a truly international meeting in the United States, most of the leading workers in the field would have to be present, and this would involve securing of funds, in dollars, enough to make possible the journey of a considerable number of scientists across the ocean. The preliminary estimate was sixteen, and this proved to be remarkably accurate: just this number had the major part of their traveling expenses paid, as well as several to whom partial payments were made, and several others who found other sources of funds, so that some twenty-five European workers were present.

Application for funds was of course made to the International Union of Pure and Applied Physics, which has at its disposal small grants which come from Unesco. Largely through the friendly interest and efforts of Professor Fleury, secretary of the union, funds to the amount of $2,500 were secured, intended principally for the traveling expenses of the members of the Commission of the Union. It was obvious from the first that the major part of the support would have to be raised from American sources, in dollars, and it was an interesting experiment in the financing and arranging of international conferences in this country to see how difficult this would be. Fortunately, we appealed to President Killian, of MIT, for suggestions, and he personally agreed to help with the securing of funds. Through his efforts, successful appeals for support were made to the Research Corporation, which made a contribution of $5,000, and Arthur D. Little, Inc., which contributed $2,500. In addition, MIT itself agreed to make a substantial contribution. Without these sources of funds, the conference could not have been held.

Announcements were sent to a fairly large mailing list, with requests for suggestions of additional persons who should be invited, and it was possible to invite all those who were thus suggested. This procedure was one which had been used for other MIT conferences on various subjects, and it seemed to be a good compromise between a very small, exclusive conference, and a very large one, announced publicly, with no semblance of an invitation list, which plan we at no time considered for the present conference. The result of this procedure was an attendance of about 200.

Two other conferences were in a way combined with this one. In the first place, the Office of Naval Research arranges small, closed conferences at periodic intervals for its contractors who are working in the low temperature field, a group which includes almost all the principal low temperature laboratories in the United States. They agreed to omit the conference which they would have scheduled at approximately the time of the MIT conference, feeling that that would adequately take its place. In addition, a group in the United States had scheduled conferences on low

temperature calorimetry at periodic intervals, often in conjunction with the ONR conferences. Members of that group were also invited and held their conference on one of the days of the larger conference, in parallel with it (see *Science,* 1949, **109**, 348). Thus a considerable number of industrial representatives were present, a fact which should prove to be a stimulus to low temperature work in industry in this country.

In arranging the program, as in choosing the membership of the conference, there were two possibilities: to have only invited papers, suggested by the committee, or to throw the meeting open to a wider group of speakers. Here again the choice was for the wider invitation, since we felt that there were many cases where speakers whose work was not well known to the committee might have important and interesting contributions to make. Accordingly the possibility was opened for any one attending the conference to suggest topics on which he would like to talk. This resulted in a rather full program; the only restriction which the committee placed on the suggested papers was that they should represent rather new work. The consensus of those attending the conference was that, though the meetings were strenuous, there was great value in getting a more complete view of the field.

The papers on the program fell naturally into a few main divisions, based on the current fields of interest in low temperature research. There was liquid helium, with numerous speculations about the theory of the superfluid phase, new results on the liquid helium film, and much new information on the behavior of the isotope He^3, which appears not to show the superfluid behavior, lending support to the hypothesis that it is the Bose-Einstein statistics (which govern the behavior of He^4 but not of He^3) which are responsible for some of the peculiar properties of liquid helium. Paramagnetic resonance, nuclear resonance, and adiabatic demagnetization came in for a good deal of discussion. There were a number of papers on superconductivity, both theoretical and experimental. Interesting low temperature effects on electrical conduction, semiconductors, diamagnetism, and various other topics were discussed; and there was a session on helium liquefiers, including a treatment of the Collins cryostat, developed at MIT during the war, and perfected by Arthur D. Little, Inc., which has furnished much of the recent impetus to the development of low temperature work in this country.

All of the topics of present interest in low temperature research were covered; and, more important, all the important laboratories were represented by their principal workers, so that discussion was lively and authoritative. Nearly everyone whose earlier work was mentioned was in the room, ready to comment on the papers. The only important group missing was that from Russia. They were invited, but sent their regrets, the term in Moscow starting too early to permit their attendance. Unfortunately, their letter of regret, enclosing numerous reprints, reached Cambridge too late to be reported at the conference, but their work was of course well known to the other research groups, and was thoroughly considered at the conference.

The members of the conference lived together at a new dormitory on the MIT campus. In many ways the most useful feature was the way in which all the workers in the field became acquainted. Many of the European visitors had opportunity not only to visit Cambridge for a longer period than the actual week of the conference, but to visit numerous other American low temperature laboratories. Such an interchange of acquaintance and ideas can well be invaluable in advancing the field of low temperature research. A number of the European workers spoke of the stimulus which they received as a result of the opportunity to hear in a connected way the whole present status of the low temperature field. Much greater than this, however, was the benefit to the American scientists, many of whom were new to the field, and who had the opportunity to come to know the masters of the subject personally and to learn at first hand of the difficulties and intricacies of research in this difficult field. The conference may well have had the effect of speeding up by a number of years the time when the many new American groups in low temperature research will get through the process of learning techniques, and will be able to start making significant contributions.

Based on the introduction to the Proceedings of the Conference on Low Temperatures, to be published shortly by the Research Laboratory of Electronics of the Massachusetts Institute of Technology.

Nancy Colloquium on the Mechanism of Carbon Combustion, September 27–30, 1949

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THE REACTION OF CARBON WITH OXYGEN is probably the chemical reaction used more than any other one. However, our knowledge of its mechanism is still surprisingly limited. It was, therefore, of great general interest that the Centre Nationale de la Recherche Scientifique of France organized a colloquium on the combustion of carbon, in Nancy, France, September 27–30, 1949. Only about twenty-five to thirty scientists invited from France, Belgium, England, and this country participated. In this way a splendid opportunity was created for a thorough exchange of opinion in a field where there are many discrepancies between the different investigations.

The colloquium was opened by Professor Terroine, president of the foreign relations commission of the center, who welcomed the visitors and stressed the importance of research and the exchange of ideas for postwar recovery.

The first day was devoted to fundamental research. R. Strickland-Constable (London), X. Duval (Nancy), and L. Meyer discussed the reactions of pure graphite with pure oxygen, CO_2, H_2O, and N_2O under such conditions (e.g., very low pressures) that one can be reasonably sure of investigating only the primary reaction of the attacking gas with the solid surface. Such investigations were initiated in 1915 by I. Langmuir, who even then found contradictory results. The discrepancies between the more recent investigations are still far beyond ordinary experimental errors and are probably due to the peculiar properties of graphite.

There exists an almost continuous transition from amorphous carbon to graphite. Graphite itself has a great tendency for "stacking errors." The graphite crystal consists of parallel planes of carbon atoms arranged in neat hexagons and held together by strong chemical bonds. Each plane is held to the next by comparatively weak van der Waals forces. Even if the planes are parallel to each other there still exists a great variety of possible "disorder" in the sense that the planes may be rotated relative to each other around the hexagonal axis.

Miss R. Franklin (Paris) reported on a very interesting systematic investigation of the transition from amorphous carbon to graphite over different crystal sizes and different degrees of order. The distance between ordered planes is 3.34 A, but increases to 3.41 A if a stacking error occurs. The apparent distance found in x-ray pictures depends on the degree of order, as it is the average of the number of ordered and the number of disordered distances.

Usually stacking errors and similar faults in crystals produce only effects of second order. In graphite they apparently have a strong first order influence on physical properties (such as electric conductivity) and kinetic behavior in chemical reactions. The amazing discrepancies in kinetic investigations (not only of the primary reaction but also technical investigations) probably must be blamed on differences in degree of order and crystal size. H. L. Riley (Worksop, England) emphasized, therefore, the necessity for a better correlation of systematic structure investigations with kinetic research.

Audubert and Busso (Paris) reported on experiments to measure and evaluate the radiative effects of the reaction $C + O_2$. Daudel (Paris) discussed the properties of graphite from the point of view of quantum mechanics, and Busso compared these calculations with experimental evidence.

The contributions about the reaction of carbon in the form of fuel under conditions similar to the technical processes revealed such an amazing amount of detailed experience (partly due to British wartime research) that it is impossible to give a short account. The group (D. T. A. Townsend, D. H. Bangham, H. C. Crone, J. R. Arthur) representing the British Coal Utilization Research Association (BCURA, Leatherhead, England) presented extensive material about widely differing problems such as the role of peroxides in the oxidation, the modes of burning of fuel, the mechanism of energy release, and the influence of impurities on the mode of combustion. The latter problem was also discussed by K. W. Sykes (Swansea, England) on the basis of wartime research results. E. Mertens (Louvain, Belgium) and Arthur (BCURA) tried to determine the primary reaction of combustion at the surface by adding substances which inhibit secondary reaction in the gas phase. Prettre (Lyon, France) gave a short report on an improved method of calculating producers.

M. Letort (Nancy) and his co-workers presented new details about the "supercombustibility" discovered by Letort. If a fuel is kept at reaction tempera-

ture in an indifferent atmosphere—such as N_2—after having burned for a time, the reactivity increases strongly. By repeating this treatment the reactivity can be increased up to five times the initial one.

Papers and discussion remarks were given in English and French. P. Goldfinger (Brussels, Belgium) acted as interpreter; his summaries in the language not used by the speaker were in themselves a valuable contribution to the discussion.

All contributions and the full discussion will be published shortly in *Journal de Physique et de Chimie*, Paris.

The organization of the meeting (by M. Letort, Nancy) was an impressive example of French hospitality. Receptions by the mayor of Nancy and the director of University of Nancy in the respective historic buildings, a night visit to the ancient Ducal Palace, visits to the modern foundry at Pont à Mousson (at whose ancient university Pere Marquette was professor before coming as an explorer to this country) and the salt mine at St. Nichlas gave excellent opportunities to get acquainted with different aspects of life in France. One was convinced that the heroic efforts of France, not only to repair the material damage of the war, but also to keep alive her great cultural tradition, have been amazingly successful.

Symposium on Brucellosis, September 22–23, 1949[1]

James T. Culbertson

National Institutes of Health, Bethesda, Maryland

A SYMPOSIUM ON BRUCELLOSIS was held at the National Institutes of Health, Bethesda, Maryland, September 22–23, 1949, under the auspices of the National Institutes of Health, the Bureau of Animal Industry, and the National Research Council. The meeting was organized by C. L. Larson, National Institutes of Health, and C. K. Mingle, Bureau of Animal Industry. Dr. Larson served as chairman at all sessions.

In the opening paper, Alice C. Evans (Bethesda, Maryland) described the early history of brucellosis and the studies which led up to the recognition, in 1918, of the relationship between the *Brucella melitensis* and *Br. abortus* organisms. Dr. Evans advised that the term *undulant fever* be dropped from medical literature because of its erroneous implications. The remainder of the first session was devoted to a discussion of brucellosis in farm animals and its control in the animal population. C. A. Manthei (Bureau of Animal Industry) reported that the artificial immunization of cattle with Strain 19 of *Br. abortus* reduced the duration and severity of the infection following exposure to virulent organisms. Aureomycin had not proved effective in the treatment of cattle infections. L. M. Hutchings (Purdue University), from study of the porcine infection with *Br. suis*, concluded that no satisfactory method of vaccination was yet available, and that the results obtained thus far with aureomycin, streptomycin, and the sulfonamides in therapy of the disease in swine have not justified field trial of these agents. W. L. Boyd (University of Minnesota) pointed out that brucellosis may occur in such farm animals as goats, sheep, horses, dogs, cats, and poultry, although only in the goat and horse, among those species just named, is the infection significant in either human or veterinary medicine in the United States. In closing the morning session, H. L. Gilman (Cornell University) emphasized the importance of the slaughter of infected animals, particularly sporadic reactors, as a method of controlling the spread of brucellosis.

In the afternoon session of the first day B. T. Simms (Bureau of Animal Industry) brought out the fact that as long as the present mobility of the livestock population is permitted, contagious diseases such as brucellosis cannot be stopped at state boundaries. The federal government must work with state livestock sanitary authorities in developing uniform methods of attack on the disease, and aid in preventing reinfection of areas where herds have been accredited. Jacob Traum (University of California) discussed *Br. abortus* Strain 19 vaccine, as used in extensive field trials in California. Robert Pennell (Sharpe and Dohme, Inc.) summarized the research on the fractionation of the *Brucellae* into constituent antigens. As yet, it is not possible to differentiate the species of *Brucella* by chemical means. Proteinnucleates of moderate serological activity and other complexes of high activity have been isolated from

(*Continued on page 485.*)

[1] The papers presented are to be published in monograph form by the American Association for the Advancement of Science.

TECHNICAL PAPERS

The Liver Cord Concept after One Hundred Years[1, 2]

Hans Elias

Communicable Disease Center, U. S. Public Health Service, Atlanta, George[3] **and** ***Department of Anatomy, Chicago Medical School***

One hundred years have passed since Gerlach (*5*) stated that the liver parenchyma consists of hepatic cells in linear arrangement. This concept has been developed, chiefly by Beale (*1*), and Pflüger (*9*), into the theory of the hepatic cord, almost universally accepted by the authors of modern textbooks. Accidentally, in the last year of its first century of existence, the validity of the liver cord theory came to be questioned.

A film strip on the histology of the liver for use in medical schools was prepared according to the description found in standard textbooks. This film strip consisted of stereograms, and presented the liver as composed of cords of cells (Fig. 1), surrounded by blood spaces called sinusoids. The liver cord is a double row of cells, as all textbooks agree (except Sharpey-Schafer's [*10*]). A bile capillary runs between the cells through the center of the cord. The textbooks have also agreed that these cell cords, columns, or trabeculae anastomose with one another and are in essence arranged radially around the central vein of each liver lobule (Fig. 2). The film strip was prepared according to these conceptions.

After completion of the stereograms for the film strip, it seemed essential to supplement the view diagrammatically presented by means of wax plate reconstructions and photomicrographs. It seemed easy to find longitudinal sections of cords (Figs. 3 and 4) in sections perpendicular to central veins. But the long rows of cells seen invariably in such sections are all too numerous. If tortuous but linear columns did exist in radial arrangement about the central vein, the chances of cutting them lengthwise with the microtome for any considerable distance are very slight. There should not be more than one longitudinal section of cord per microscopic field at high power, even in sections perpendicular to a central vein. The majority of sections of cords, if they did exist, should be oblique, i.e., about three to four cells long. While it had been suspiciously easy to find apparently longitudinal sections of cords, it was practically impossible to find cross sections of cords, despite the fact

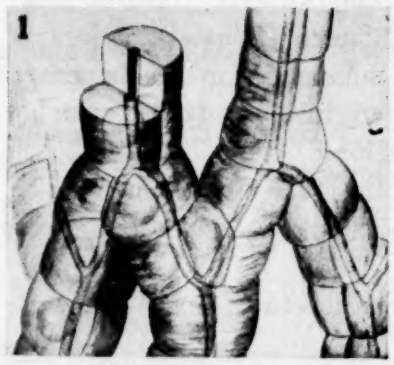


FIG. 1. Stereogram of the conventional concept of anastomosing liver cords, containing axial bile canaliculi.

FIG. 2. Conventional interpretation of the structure of an hepatic lobule.

FIG. 3. Section of human liver lobule perpendicular to central vein. The arrow points to a cross section of an interlaminary bridge.

FIG. 4. Section of liver lobule of horse, perpendicular to central vein. This, as well as the preceding section, consists of long rows of cells, one cell wide.

that they should be very numerous in tangential sections of a lobule (see Fig. 2). In man (Fig. 5) and the cat, sections parallel to the central vein showed long rows of cells simulating cords.

In whatever direction we may cut a human or cat liver lobule, we always obtain long linear rows of cells. And at widely scattered places we find isolated patches of hepatic cells in an epithelial arrangement (Fig. 6).

The liver of man and the cat, hence, can *not* consist of cords or columns. It must be fashioned of structures which will show, in almost whatever direction they are sectioned, long rows of cells one cell wide, and which at a few places may give sections appearing as larger epithelial masses. Clearly, the formations that can yield these sections cannot be anything but curved plates, sheets, or laminae, one cell thick.

This logical conclusion can easily be verified by means of wax plate reconstructions and also by examining thick, unstained sections mounted in glycerin. Fig. 7 shows a group of hepatic plates drawn with the camera lucida from a section perpendicular to a central vein and 30 μ thick. The plates are curved, frequently perforated, and

[1] The contents of this report are being described and discussed more thoroughly in the *American Journal of Anatomy*. The paper was read before the American Association of Anatomists, April 23, 1948, and abstracted in *Anat. Rec.*, 1948, **100**, 23.

[2] A preliminary report.

[3] From Production Division, Atlanta, Georgia.

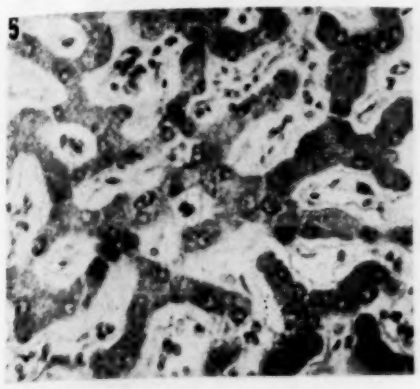
wherever they are cut the section is a row of cells one cell wide. It is clear that sections cutting these sheets in other directions will appear as single rows of cells. But it will also be possible, if the level and angle of cutting is favorable, to obtain tangential sections of these laminae, such as the section shown in Fig. 6. In the upper left-hand corner of Fig. 7, unicellular bridges can be seen connecting neighboring plates. Such unicellular and sometimes bicellular bridges account for the occasional occurrence of apparent cross sections of cords, as seen at the tip of the arrow in Fig. 3.

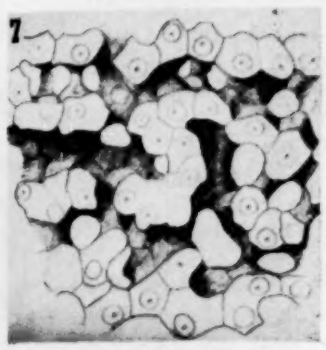
Fig. 8 shows a 30-μ-thick section parallel to a central vein, from the same cat's liver as seen in the preceding figure. It shows that the tortuous laminae anastomose with one another and form wide tubes with irregularly curved, and undoubtedly flexible, walls.[4] We are looking into these saccular lacunae which run toward the central vein. But we are following their course for a distance of only 30 μ. Wax plate models showing the same arrangement were obtained by taking serial, optical sections with the motion picture camera, moving the microscope's fine adjustment from frame to frame for 1 μ.

For the conventional idea of the hepatic cord, consisting of a double row of cells (Fig. 1), there must, therefore, be substituted the concept of the hepatic lamina (Fig. 9) which is one cell thick and frequently perforated to permit the passage of sinusoids. However, hepatic sheets are not straight and they never occur isolated. They form a continuous tissue of connected walls which enclose between them the spaces in which the sinusoids run. This arrangement is shown schematically in Fig. 10. In reality, the plates are curved as seen in Fig. 11. The liver of man and the cat may tentatively be called the saccular type of liver, since the spaces between the plates have the character of long sacs.

A second type of liver is to be found in the horse and in the rabbit. This is the tubular liver, one in which the spaces between the hepatic laminae or walls are narrow and cylindrical. Fig. 13 shows some of these tubes from a horse, cut longitudinally, that is, perpendicularly to the central vein of the lobule. Fig. 13 is also a 30-μ-thick section, unstained, and mounted in glycerine. As Fig. 4 shows, this tubular type of liver also yields, in thin sections, long single rows of cells. Fig. 12 is a stereogram of the tubular liver.

The knowledge of the laminary liver structure makes it much easier to understand the existence of the well-known polygonal meshwork of bile capillaries. These polygonal networks, which are so easily demonstrated with Golgi's silver method or with Chrzonszczewsky's indigocarmine method, do not fit at all into the conven-





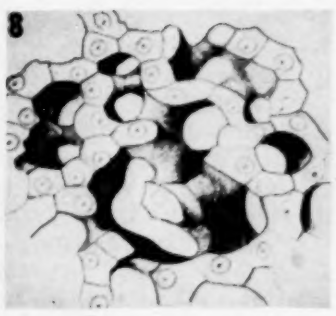
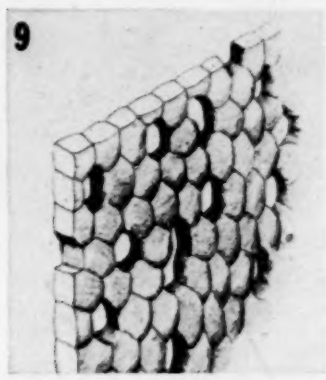


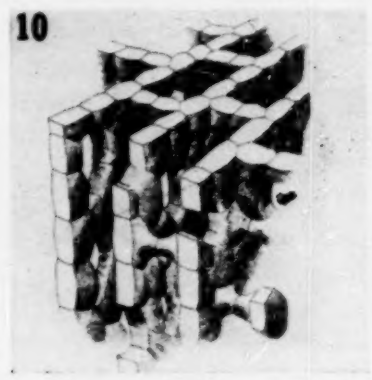
FIG. 5. Section of human liver lobule parallel to central vein. Also in this direction, long rows of cells, one cell wide, are seen. Note the incompatibility of this as well as of the succeeding section with Figs. 1 and 2.

FIG. 6. Tangential section of liver plate (lamina hepatica) of cat.

FIG. 7. Liver of cat, showing anastomosing, cribriform, curved, hepatic plates. The spaces which they surround are irregular in shape. Camera lucida drawing of frozen section, 30 μ thick, cut perpendicularly to central vein, not stained, mounted in glycerin.

FIG. 8. Liver of cat, showing anastomoses of hepatic plates, saccular shape of hepatic lacunae, interlaminary bridges (upper left), perforations of laminae. Technique as in Fig. 7, sectioned parallel to central vein.





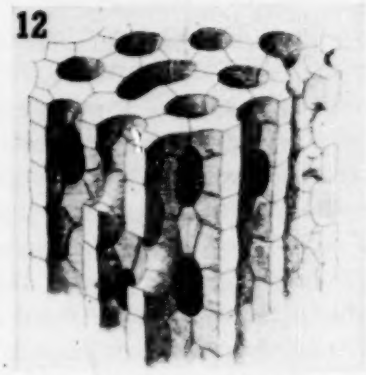


FIG. 9. Schematic stereogram of hepatic plate.

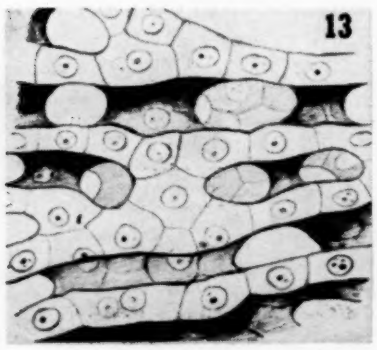
FIG. 10. Schematic stereogram of the saccular liver. Note interlaminary bridges.

FIG. 11. Semidiagrammatic stereogram of the human liver (hepar sacculare).

FIG. 12. Stereogram of the equine liver (hepar tubulare).

[4] The flexibility of the hepatic laminae becomes obvious in sections of livers from which the blood has been drained, after severe hemorrhages. In this case the lacunae become collapsed.

tional picture of columnar liver structure. The bile capillaries run, in the majority of cases, between the cells and form hexagonal, intercellular, and pericellular networks within the liver plates.



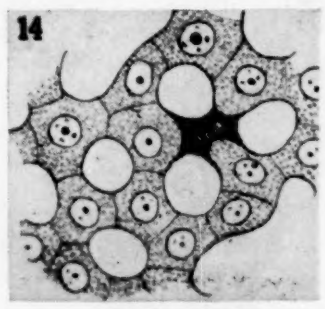


FIG. 13. Liver of horse, showing the tubular character of the lacunae, technique as in Figs. 7 and 8, perpendicular to central vein.

FIG. 14. Same as Fig. 13, but cut parallel to central vein.

From a historical point of view these observations are very interesting. The earliest account of the intimate structure of the liver was given by Gerlach in 1849 and upheld by Beale in 1856 (*1*). Beale described the liver as consisting of membranous tubes which contained rows of liver cells. Basically, this is the conception of modern textbooks. A cell cord surrounded by a network of lattice fibers is essentially the same as a row of cells contained in a noncellular tube.

In 1859, Budge (*4*) raised doubts. He objected to the inadequate methods Beale had used (maceration for days in water, not even in saline, at room temperature; teasing; no use of sections), but he did not find the correct solution.

In 1866, Hering (*6*) analyzed and described, in all correctness and exactly as outlined in this paper, the tubular type of liver (not using this expression) in the rabbit.

In 1867, Kölliker (*8*) described the human liver as a network of tender leaves and strings. Thus, he took a position intermediate between Beale and the view presented in this paper.

In 1869, Pflüger (*9*) ridiculed Hering and upheld the opinion of Gerlach and Beale. Both Beale and Pflüger were prominent men; hence their view was almost universally accepted. Pflüger ignored Kölliker's findings.

Once more, in 1872, Hering (*7*) asserted his viewpoint, bringing more evidence and considering very carefully the geometrical, three-dimensional aspect of the subject. However, his opinion was not accepted.

As late as 1889, Beale (*2*) restated and tried to prove his original concept by obsolete methods; washing the liver through the portal vein with water (not saline), then letting it soak for some time in soda water before beginning to inject his material and observe it with the teasing method. Inadequate as his methods were, Beale nevertheless prevailed, and the most modern textbooks presented the mammalian liver as made of cell columns ensheathed in an argyrophil network.

There are two exceptions to the majority of opinions in textbooks: Braus (*3*) found plates in the liver of man. These are, however, according to him, two cells thick and occur simultaneously with cords which are also two cells thick. The other exception is Sharpey-Schafer (*10*), who described the liver as a parenchyma pervaded by cylindrical sinusoids. This is almost in accordance with Hering, from whom a figure is reproduced. But Sharpey-Schafer does not mention the fact that the walls separating the sinusoids are only one cell thick.

All other textbooks have accepted without question the statements of Beale and Pflüger and perpetuated for 99 years a fundamental misconception of this important organ.

It was the problem of stereographic screen projection of liver anatomy which stimulated a re-examination of the architecture of this organ.

References

1. BEALE, LIONEL S. *Med. Times Gaz.*, 1856, **12**, 455.
2. ———. *The liver.* London: J. & A. Churchill, 1889
3. BRAUS, HERMANN. *Anatomy des Menschen.* Berlin: J. Springer, 1924. Vol. 2.
4. BUDGE, J. *Müller's Arch.*, 1859, 642.
5. GERLACH, J. *Beiträge zur Strukturlehre der Leber.* Mainz: E. Janitsch, 1849.
6. HERING, EWALD. *Sitzber. Akad. Wiss., Math. Naturwiss.*, 1866, **54**, 496.
7. ———. *The liver.* In Salomon Stricker, *Manual of human and comparative histology.* London: The New Sydenham Society, 1872. Vol. 2, pp. 1–48.
8. KÖLLIKER, A. *Handbuch der Gewebelehre des Menschen.* (5th Ed.). Leipzig: W. Engelmann, 1863.
9. PFLÜGER, E. *Arch. f. d. ges. Physiol.*, 1869, **2**, 459.
10. SHARPEY-SCHAFER, E. In H. M. Carleton (Ed), *Essentials of histology.* (14th Ed.). Philadelphia: Lea & Febiger, 1938.

Perchloric Acid in the Cytochemistry of Pentose Nucleic Acid[1]

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Cytochemical methods for demonstrating pentose nucleic acid (PNA)in tissue sections are based on the removal by ribonuclease of the ability of certain cell constituents to be stained by basic dyes, such as pyronin in Unna's pyronin-methyl green mixture (*1*), methylene blue (*3*), and toluidine blue (*4*). Parallel sections, treated and not treated with ribonuclease, are stained simultaneously and compared. The specificity of the demonstration, of course, depends upon the freedom of the enzyme preparation from proteolytic activity.

Analytical work in this laboratory (*5*, *6*) on root tip homogenates has led to a method for the differential acid extraction of the constituents of PNA and desoxypentose nucleic acid (DNA) into separate fractions, which may

[1] This work was supported by grants from the American Cancer Society, upon recommendation of the Committee on Growth of the National Research Council, and from the National Cancer Institute.

then be assayed by spectrophotometric or other means. In the procedure used, the homogenate, after preliminary extraction with acidified alcohol, alcohol-ether, and cold 2% perchloric acid, is treated with 10% perchloric acid

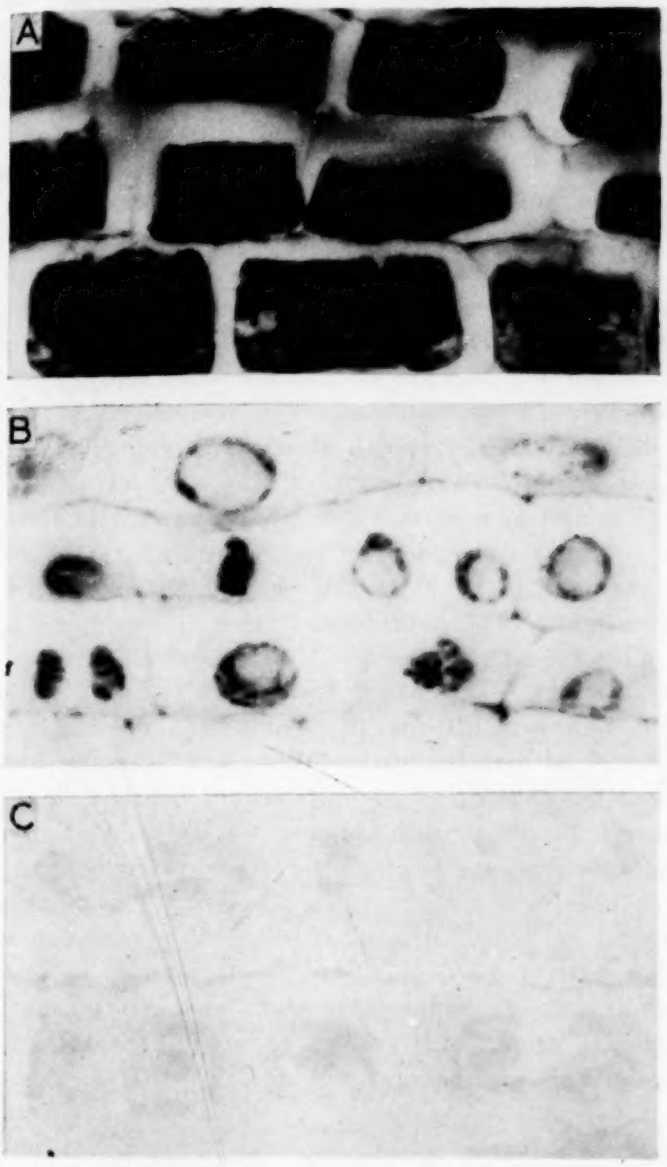

FIG. 1. Meristematic cells from nearly adjacent sections of a single primary root tip of *Zea mays*, fixed in ethyl alcohol, stained simultaneously in toluidine blue, and photographed identically, × 1,000. A—Untreated cells. B—Cells treated with 10% perchloric acid for 18 hr at 4° C. C—Cells treated with 5% perchloric acid for 20 min at 70° C.

for 4–18 hr at 4° C. This treatment removes PNA but not DNA from the residue, and appears to have practically no proteolytic effect. Subsequent treatment with perchloric acid at an elevated temperature removes DNA from the residue.

Similar treatment of sectioned material might be expected to have similar effects. In particular, it should be possible to use cold perchloric acid, instead of ribonuclease, together with a basic dye to demonstrate PNA in sections. We have attempted such a demonstration in root tip sections. Although several problems, mainly of fixation, require more study, the results to date appear to be of sufficient interest to warrant publication.

In the experiments reported here, primary root tips of *Zea mays* seedlings have been fixed in 75% alcohol for 48 hr, dehydrated, embedded in paraffin, and sectioned longitudinally at 8 μ. The paraffin ribbon obtained from each root tip was cut into segments of two sections. Segments from a single root tip were attached alternately to each of a set of slides, which were then passed through xylene and alcohols to water. This treatment undoubtedly removes water and fat-soluble constituents from the sections, as does the preliminary extraction of the homogenate in the analytical procedure. One or more slides from a root tip were then placed in 10% perchloric acid for 18 hr at 4° C, while control slides were kept in water at the same temperature. Other slides were placed in 5% perchloric acid for 20 min at 70° C, with control slides being given similar exposure to water. All the slides of a set were then washed in water, fastened into a wire clip (*2*), dehydrated together in 70% alcohol, and stained for 3 hr in a 1% solution of toluidine blue O in 75% alcohol. They were destained and dehydrated on the clip, by passing quickly through normal butyl alcohol to xylene, and mounted. Similarly treated slides, which have been passed through the solutions on the same clip, appear to be quite uniformly stained. Sections treated in this way were photographed with a red filter (Corning 2018) at the same exposure and printing times, and at a relatively low degree of contrast (Eastman Panatomic-X film and D-76 developer, No. 1 bromide paper).

Fig. 1, of meristematic cells of a single root tip, illustrates the results of this procedure. Ethyl alcohol is admittedly not a good fixative for plant material. It produces a great deal of shrinkage of the cell as a whole, and does not yield a good image of either cytoplasmic or nuclear structures. From a cytochemical point of view, however, the fixation is simple, and the effect of the perchloric acid treatment is clear. In Fig. 1A, of control cells treated with cold water, the cytoplasm, nucleoli, and chromosomes are strongly stained. In Fig. 1B, of cells treated with cold perchloric acid, neither cytoplasm nor nucleoli stain appreciably, while the chromosomes stain well. In agreement with the analytical findings, treatment of sections with 5% perchloric acid at 70° C for 20 min leaves the cells practically unstainable with toluidine blue (Fig. 1C), whereas the hot water control is indistinguishable from Fig. 1A. Similar sections have been stained by the Feulgen reaction. The chromosomes are Feulgen-positive before and after treatment with cold perchloric acid. After treatment with hot perchloric acid, no staining whatever occurs.

It appears from these results that treatment of sectioned material with cold and hot perchloric acid should be useful in the cytochemistry of the nucleic acids. The cold perchloric acid treatment, which appears to be quite comparable to that by ribonuclease, is of particular interest in the demonstration of PNA.

References

1. BRACHET, J. *Comp. rend.*, 1940, **133**, 88.
2. CHAMBERLAIN, CHARLES J. *Methods in plant histology.* (5th Ed.) Chicago: Univ. Chicago Press, 1933. P. 15.
3. DEMPSEY, E. W. and WISLOCKI, G. B. *Physiol. Rev.*, 1946, **26**, 1.
4. OGUR, M. and ROSEN, G. U. *Fed. Proc.*, 1949, **8**, 234.
5. *Arch. Biochem.* In press.

Molecular Rearrangements in the Sterols V. The Mechanism of Formation of *i*-Cholesterol

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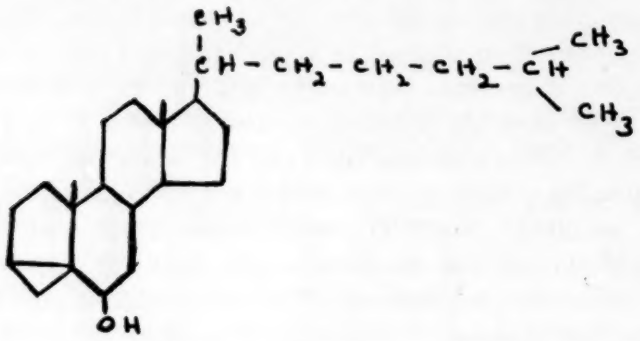
Frick Chemical Laboratory, Princeton University

The present accepted structure of *i*-cholesterol (I) was first proposed by Wallis, Fernholz, and Gephart (*9*).



(I)

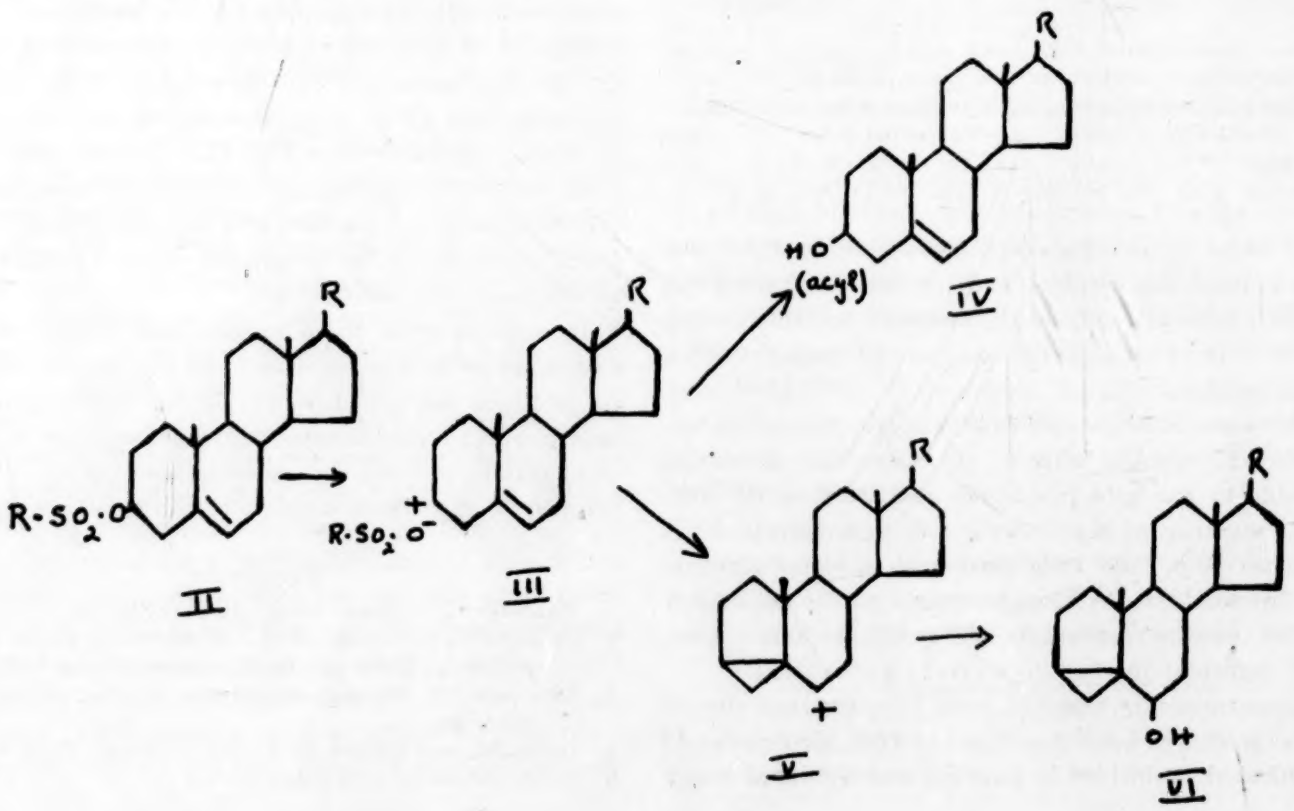
Proof of this formulation has been submitted by Ford, Chakravorty, and Wallis (*4*) and by Ladenberg, Chakravorty, and Wallis (*6*). These investigators (*4, 6, 9*) prepared mixed acetates of cholesterol and *i*-cholesterol by heating cholesteryl *p*-toluenesulfonate with acetic anhydride in the presence of potassium acetate. A mixture of the two isomers was also prepared by Benyon, Heilbron, and Spring (*1*) by hydrolysis of the same ester in aqueous acetone containing potassium acetate. In recent years it has been shown that the formation of *i*-steroids is a general property of all steroid compounds having a hydroxyl group in the β configuration at C_3 and a double bond at C_5—C_6 (*5*).

In this paper we wish to describe the results of certain studies we have made on the mechanism of this isomerization. For this purpose we have carried out the hydrolysis of a series of four sulfonic acid esters of cholesterol with different electron-donating powers in the acid residue. In order further to elucidate the mechanism we have varied the conditions of hydrolysis in the case of one of these esters—cholesteryl *p*-toluenesulfonate. The results of these studies are described in this paper.

From these results it is concluded that rearrangement of II is preceded by ionization—a conclusion in agreement with a mechanism recently proposed by Weinstein and Adams (*10*). The cholesteryl ion III is then either attacked by a negative ion and converted into the normal structure IV or it undergoes a rearrangement to the *i*-cholesteryl ion V, which is similarly converted to the isomeric sterol VI.

The first step of this process is slow. It is followed by a rapid conversion or rearrangement to the corresponding alcohols. On this concept the mechanism is, therefore, of the unimolecular type.

The breaking of the C—O linkage with ionization instead of the S—O linkage agrees with the work of Ferns and Lapworth (*3*) on the hydrolysis of sulfonic acid esters, and with the work of Phillips (*7*) on the Walden inversion of the *p*-toluenesulfonate of benzyl methyl carbinol. In the present instance we believe that our results show that the cholesteryl ion rearranges to the *i*-cholesteryl ion while still a free ion. This rearrangement may be looked upon as being the result of an electron shift at the double bond C_5—C_6 towards C_5, thereby binding the cationic center at C_3 and creating another cationic center at C_6. The negative hydroxyl ion or acetate ion, as the case may be, then attacks this new



center, forming *i*-cholesterol or its acetate. If such an attack occurs before the rearrangement of the cholesteryl ion then the normal product is produced.

It should be pointed out that this polarizability of C_5—C_6 double bond is in agreement with the interpretation of Shoppee (*8*) in explaining certain replacement reactions at the C_3 position of cholesterol, which takes place with retention of configuration while the same reactions with dihydrocholesterol result in a Walden inversion.

TABLE 1

PARTIAL HYDROLYSIS, UNDER IDENTICAL CONDITIONS, OF FOUR CHOLESTEROL ESTERS*

Ester	Wt in g	Wt of digitonide	% hydrolysis	K_1/sec
p-Methoxybenzene sulfonate	1.3920	1.2800	68.5	3.2×10^{-4}
p-Toluenesulfonate	1.3520	0.4992	87.1	5.7×10^{-4}
Benzenesulfonate .	1.3170	0.3968	90.1	6.7×10^{-4}
p-Nitrobenzene-sulfonate	1.4296	0.2976	92.6	7.2×10^{-4}

* One four-hundredth mole of the ester and 8/400ths mole of KOAc was refluxed for 1 hr at constant temperature with 50 ml of 90% aqueous acetone. The unhydrolyzed material was completely hydrolyzed with aqueous acetone and a very small amount of KOH. Cholesterol was precipitated as digitonide.

This unimolecular mechanism is also in accordance with the observation (*1*, *4*, *6*) that no trace of epicholesterol is detected, indicating that the replacing ion does not attach itself to the ester directly, but after ionization. Similarly, the very presence of rearrangement points to a unimolecular mechanism according to the views of Dostrosky and Hughes (*2*).

In Table 1 we have recorded results obtained in partial hydrolysis, under identical conditions, of four cholesteryl esters. The rates of hydrolysis show that the strength

TABLE 2

HYDROLYSIS OF THE FOUR ESTERS IN TABLE 1, CARRIED TO COMPLETION*

Ester	Wt of digitonide in g	% Conversion to *i*-cholesterol
p-Methoxybenzenesulfonate	0.4764	88.1
p-Toluenesulfonate	0.4840	87.9
Benzenesulfonate	0.4800	88.0
p-Nitrobenzenesulfonate	0.4832	87.9

* One four-hundredth mole of the ester and 8/400ths mole of KOAc were boiled for 3 hr with 50 ml of 70% aqueous acetone and then hydrolyzed with alcoholic KOH.

of the C—O bond is dependent, as expected, on the electron-donating power of the acid residue. The elimination of the sulfonate ion is facilitated by the weakened C—O bond, and is inversely proportional to the electron-donating power of the acid residue. Although these results can be reconciled with either a unimolecular or a bimolecular mechanism of hydrolysis, the fact that no traces of epicholesterol are formed strongly indicates that the replacing ion does not attack the ester directly and the reaction is consequently unimolecular in nature.

This point of view is definitely shown when the hydrolysis of the same four esters is carried to completion. In this case we find that the acid residue has no influence on the proportion of rearrangement, as is shown in Table 2. The rearrangement takes place in the cholesteryl ion after loss of the acid residue, which then has no effect on isomerization.

It is also of interest to observe the effect on isomerization of varying the water content of the hydrolyzing medium. This effect is shown in Table 3. Similarly, the influence of the potassium acetate concentration is shown in Table 4. Our results show that the lower the concentration of the active reagent, i.e., water and potassium acetate, the higher is the yield of *i*-cholesterol. Again this is understandable in the mechanism described above. The cholesteryl ion has a better chance to rearrange before the attack of the reagent.

TABLE 3

EFFECT ON ISOMERIZATION OF VARYING THE WATER CONTENT OF THE HYDROLYZING MEDIUM*

Concentration of water	Concentration of acetone	% *i*-Cholesterol
5%	95%	89.4
35%	65%	88.3
50%	50%	86.5

* One four-hundredth mole of cholesteryl *p*-toluenesulfonate was completely hydrolyzed in acetone solution in presence of 8/400ths mole of KOAc.

TABLE 4

EFFECT ON ISOMERIZATION OF VARYING THE POTASSIUM ACETATE CONCENTRATION*

Moles KOAc	% Conversion
2	89.5
8	87.9
32	87.0

* *p*-Toluenesulfonate was completely hydrolyzed in 80% aqueous acetone in presence of varying concentrations of KOAc.

There are, however, other alternatives for the rearrangement:

1. The rearrangement occurs concurrently with ionization. This case is very unlikely because the electron-donating power of the acid residue, which governs the ionization, does not affect the proportion of isomerization (Table 2).

2. The rearrangement takes place concurrently with addition of the negative ion at the moment of its attack. This case is unlikely because the proportion of isomerization is changed by changing the water or acetate concentration and it is not apparent how this change would affect isomerization taking place at the moment of attack by a single ion.

3. The cholesteryl ion, first formed, rearranges to form the *i*-cholesteryl ion, but (a) the rearrangement does not reach equilibrium, or (b) the rearrangement does effectively reach equilibrium but the proportion of the two ions is changed by a change in the medium.

It is not possible to eliminate possibilities (a) or (b) on the basis of the data presented here. However, if rearrangement of the cholesteryl ion can be thought of as an intramolecular reaction between an ion (the carbonium ion at C_3) and a neutral molecule (the double bond at C_5—C_6), alternative (b) can be eliminated, because there is no primary salt effect in such a reaction.

References

1. Benyon, J. H., Heilbron, I. M., and Spring, F. S. *J. chem. Soc.*, 1937, **140**, 1459.
2. Dostrovsky, J. and Hughes, E. D. *J. chem. Soc.*, 1940, **143**, 167.
3. Ferns, J. and Lapworth, A. *J. chem. Soc.*, 1912, **101**, 273.
4. Ford, E. G., Chakravorty, P., and Wallis, E. S. *J. Amer. chem. Soc.*, 1937, **59**, 1415; 1938, **60**, 413; 1939, **61**, 3483.
5. Gilman, Henry. *Treatise of Organic Chemistry*, 1943, 2nd ed., **2**, 1383.
6. Ladenburg, Kurt, Chakravorty, P., and Wallis, E. S. *J. Amer. chem. Soc.*, 1939, **61**, 3483.
7. Phillips, Henry. *J. chem. Soc.*, 1923, **123**, 44.
8. Shoppee, C. W. *J. chem. Soc.*, 1946, **149**, 1147.
9. Wallis, E. S., Fernholz, E., and Gephart, F. T. *J. Amer. chem. Soc.*, 1937, **59**, 137.
10. Weinstein, S. and Adams, R. *J. Amer. chem. Soc.*, 1948, **70**, 838.

Degradation of Glucose-1-C^{14} and a Possible New Step in the Mechanism of Fermentation[1]

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The availability of glucose-1-C^{14} [3] has permitted the verification of a scheme of glucose degradation applied to sugars formed in photosynthesis to determine the distribution of isotopic carbon within the sugar (*1*). As a result of the present investigation, there appears to be a second, though minor, pathway of fermentation by the test organism, *Lactobacillus casei* ε.

The degradation procedure is depicted in Fig. 1 and involves the following sequence of reactions: the fermentation of glucose to lactic acid; the oxidation of lactic acid to carbon dioxide (carbons three and four of the original glucose) and acetic acid; the pyrolysis of barium acetate to acetone and barium carbonate (carbons two and five); and the formation of iodoform (carbons one and six) from the acetone. The various steps in the process have been tested with (a) methyl- and carboxyl-labeled acetate, (b) α-labeled lactate,[4] and (c) glucose-1-C^{14}.

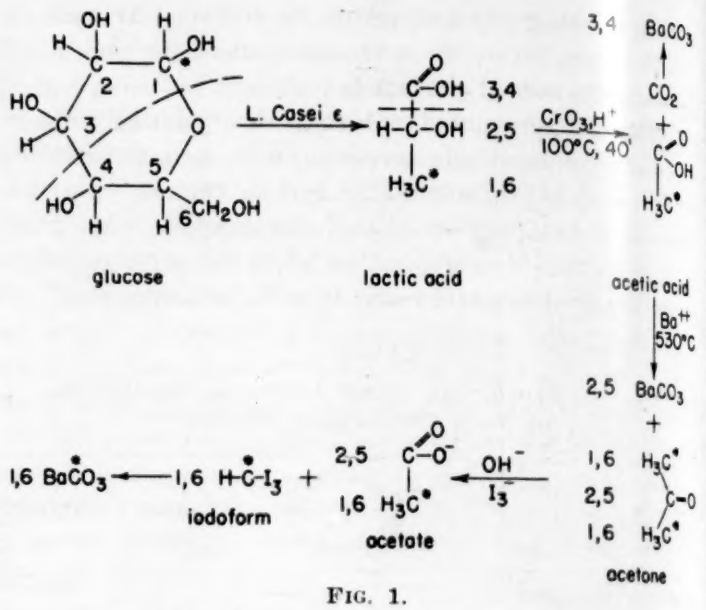

FIG. 1.

Both methyl- and carboxyl-labeled acetates have been pyrolyzed under a variety of conditions, a number of which can be so unfavorable (e.g., that at 450° C with flowing argon), as to result in the appearance of more than 8% of the activity of methyl-labeled acetate in the purified residual barium carbonate. Under similar conditions, with carboxyl-labeled acetate, less than ½ of 1% of the activity is in the iodoform. Using flowing argon, at a temperature of 530° C for 10 min, and liberating the carbon dioxide from the residual carbonate with lactic acid, only 0.86% of the activity of the methyl-labeled acetate is found in the carbonate.

Electrolysis of sodium acetate (*2*), the products of which are carbon dioxide and ethane, resulted in 1% of the radioactivity of the methyl group in the carbon dioxide from methyl-labeled acetate.

The chromium trioxide oxidation of α-labeled lactate (*7*) purified and recrystallized as the zinc salt, resulted in 4.3% of the activity in the carbon dioxide evolved. This activity arises primarily from oxidation of compounds other than the acetic acid itself, as less than 1% of the theoretical barium carbonate arises from acetic acid under identical conditions.

The degradation of lactate from the bacterial fermentation, similarly isolated and purified as the zinc salt, resulted in 9.3 ± 0.1% of the activity in the barium carbonate, rather than the empirical 4.3 ± 0.1. The difference of 5% must be ascribed to the activity of carbons three and four. In fermentation the carboxyl group of the lactic acid is presumed to arise solely from carbons three and four of glucose, whereas in ordinary chemical

[1] This work was supported in part by funds from the Atomic Energy Commission. The research was initiated at the Radiation Laboratory, University of California, and continued at the Ames Laboratory, Iowa State College.

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[3] The glucose used by the senior author was synthesized by John C. Sowden (*6*); that used in the California laboratory was synthesized by H. Mahler according to Sowden's general method (*4*).

[4] The acetates were synthesized by Dr. B. Tolbert, and the lactic acid by Dr. R. Lemmon (both at the University of California Radiation Laboratory).

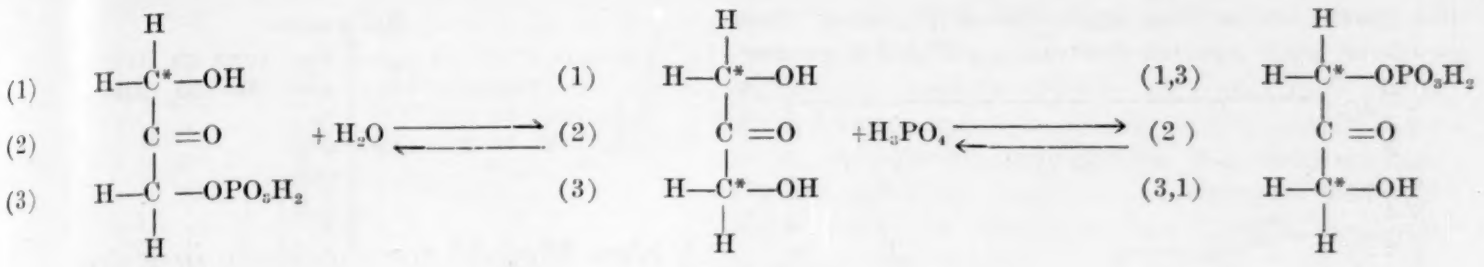

FIG. 2.

alkaline rearrangements resulting in lactic acid (*5*) the carboxyls may stem from carbons one and six. This is certainly so in the case of gluconic acid, where 1.3 moles of lactic acid per mole of gluconic acid can be obtained.

These experiments are in general accord with the recent note by Koshland and Westheimer (*3*) on the fermentation of glucose-1-C^{14} by yeast. In yeast fermentation, the carbon dioxide may be derived from a variety of intermediary products of metabolism. Furthermore, the ability of yeast to fix carbon dioxide is pronounced. In view of these and the rather large counting deviations in their experiments, a quantitative interpretation of their results is not possible.

At least three apparent explanations of our results are suggested, the latter two of which represent deviations from the normal fermentation mechanism: (a) that metabolic carbon dioxide (from the radioglucose) is reversibly reincorporated in lactic acid, in which case the activity would reside primarily in the carboxyl group, (b) that there is a fermentation mechanism in which carbons one and six rather than three and four are oxidized, (c) during the course of the normal fermentation mechanism the triose phosphates are in (partial) equilibrium with the free trioses, as in Fig. 2. This would, of course, result in activity appearing in both carbons one and three of lactic acid, to an extent depending on the amount of approach to an equilibrium. We have not investigated the enzyme systems involved and are thus not able to choose between the second and third explanations. The first explanation appears difficult to reconcile with the fact that the fermentation is conducted in M/10 bicarbonate (pH 6.8), with subsequent dilution of carbon dioxide formed by metabolism. Furthermore, control experiments with inactive glucose but with radiocarbonate (2.8×10^6 disintegrations per min in 5 ml of reaction mixture) resulted in approximately 0.02% of the activity in the lactic acid formed. There still remains the possibility, albeit dubious, that the metabolic carbon dioxide is preferentially used at the site of formation, prior to the diffusion and subsequent dilution effects of the solution's inactive bicarbonate.

References

1. ARONOFF, S., BARKER, H. A., and CALVIN, M. *J. biol. Chem.*, 1947, **169**, 459.
2. HOLEMANN, P. and CLUSIUS, K. *Z. f. physik. Chem.*, 1937, **35B**, 261.
3. KOSHLAND, D. and WESTHEIMER, F. H. *J. Amer. chem. Soc.*, 1949, **71**, 1139.
4. MAHLER, H. *U.C.R.L.—174, The synthesis of* D-*Glucose-1-C^{14}.*
5. MONTGOMERY, R. *Scientific Report*, No. 11. Sugar Res. Foundation, January, 1949.
6. SOWDEN, JOHN C. *Science*, 1949, **109**, 229.
7. SZEBER'ENYI, P. *Z. anal. Chem.*, 1917, **56**, 505.

Diurnal Responsiveness of *Erysiphe graminis* to Nutrients

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Because it can be grown readily on barley in the greenhouse throughout the year, because the conidia germinate well on various culture media, and because germ tube growth is sharply increased by a number of nutrient chemicals, the fungus *Erysiphe graminis* D.C. appears well suited to studies of growth *in vitro* of a powdery mildew. No member of the Erysiphaceae has been cultured on a nonliving substrate, and all are commonly regarded as obligate parasites.

Previous studies of *Erysiphe graminis* have indicated no diurnal cycle in the germinability of the conidia or in the division of the generative cell such as is exhibited by *Erysiphe polygoni* (*1*). On an agar substrate, germ tube growth of *E. graminis* is increased by crude animal and plant extracts, especially a solution of hen's egg yolk; by iron sulfate, sucrose (*2*), disodium α-tocopherol phosphate, and Tween 60 (*4*). More recently, a medium consisting of 0.8% agar, 0.6% fructose, and 0.003% animal lecithin has been used. Using this medium, there has been great variation in germ tube length in different trials; such variation being associated, apparently, with the time of day the trials were started.

To check on the effect of time, plates of plain agar and lecithin-fructose agar, prepared simultaneously and stored at 0° C, were seeded with conidia of *E. graminis* from greenhouse plants at various times throughout the day and incubated at 16° C in the dark, and the germ tube growth was measured 5 days after seeding. Results of the first trial, started on February 11, 1949, are presented in Fig. 1, and indicate that germ tube growth on nutrient agar was greatest from seedings made about 12 M., and poorest from seedings made at 8:30 A.M. The germ tubes of the 12:30 P.M. seeding were 85 μ greater or 175% greater than the germ tubes of the 8:30 A.M. seeding. Germ

tube growth on nutrient agar was 40% greater than growth on plain agar at 8:30 A.M., and 236% greater

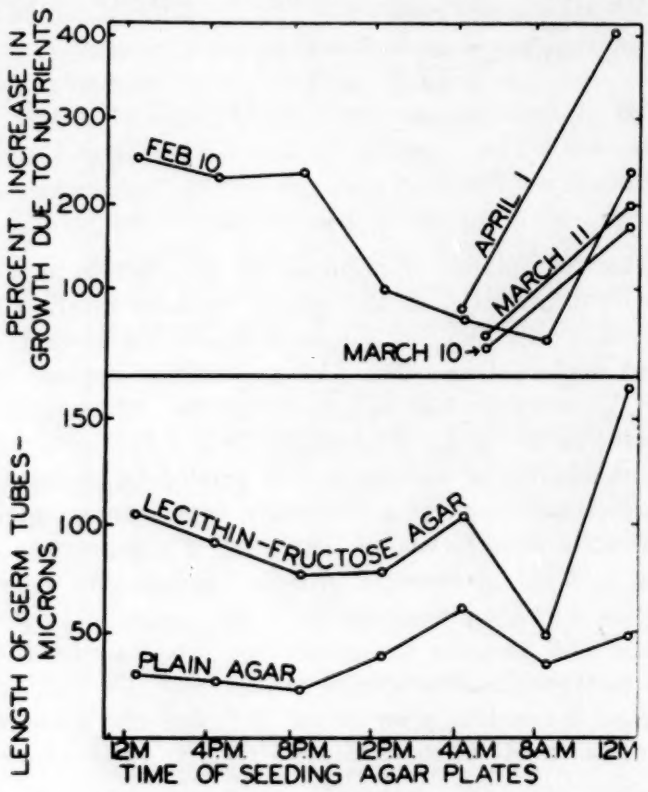

FIG. 1. Diurnal cycle of responsiveness of conidia of *Erysiphe graminis* to lecithin-fructose agar. Each value is the average of 20 germ tubes. Data of the 2 lower graphs are for February 10–11, 1949. The weather was cloudy before and during these tests and also during those of March 10 and 11. The weather was clear before and during the test of April 1.

than on plain agar at 12:30 P.M. In three subsequent trials, an attempt was made to compare only the approximate peak and trough of this diurnal cycle of responsiveness to nutrients. In these trials, the increase in germ tube length due to nutrients averaged 56% for seedings made from 4:15 to 5:30 A.M., and 244% for seedings made from 11:50 A.M. to 12:20 P.M. (see Fig. 1). In all trials, germination ranged between 25% and 92% and showed no apparent diurnal cycle and no apparent correlation with germ tube length.

Concurrent attempts to study in a similar way germ tube growth *in vitro* of *Erysiphe polygoni* from bean, and *Uncinula necator* from grape (both fungi show a clear diurnal cycle of conidiophore maturation, and the bean fungus also shows diurnal variation in sensitivity to copper sulfate [*3*]) failed to evince any marked response to the lecithin-fructose medium at any time tested. This indicates that there are specific differences between closely related species of powdery mildews with respect to nutrient response.

As the principal index of the nutrient value of a test chemical is the difference in growth of germ tubes on media with and without the test chemical, this diurnal responsiveness to nutrients, as in the germinability of the conidia of *E. polygoni* (*1*), is apparently the result of diurnal changes in the fungus while grown on the host plant before use in these tests. Diurnal responsiveness may be of considerable importance in studies of the growth *in vitro* of some powdery mildews.

References

1. YARWOOD, C. E. *J. agric. Res.*, 1936, **52**, 645.
2. ———. *Phytopathology*, 1941, **31**, 865 (Abstract).
3. *Ibid.*, 1945, **35**, 895.
4. *Ibid.*, 1948, **38**, 920 (Abstract).

A New Method for the Study *in Vitro* of Rumen Digestion

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The utilization of cellulose has long been recognized as a primary factor in the nutrition of ruminants and particularly so among those subsisting largely on roughages. It is believed, therefore, that the rate of cellulose digestion *in vitro* by the microorganisms of the rumen may be used as a measure of how nearly it is possible to approach cellulose digestion *in vivo*.

A technique for the study of digestion in the rumen *in vitro* has recently been published by Marston (*3*). His procedure appears to simulate more closely the natural environment in the rumen than any earlier investigations. There is, however, one significant omission. No provision is made for the removal of nongaseous fermentation products which, as they accumulate, might be expected to slow the rate and eventually inhibit digestion.

To test the influence of such an accumulation of breakdown products, cellulose digestion was compared after incubation with fresh rumen fluid in a closed vessel (the technique of Marston), and in a semipermeable bag suspended in a large volume of aqueous growth medium.

Samples of rumen liquor were collected by scooping semisolid material from the lower regions of the rumen of a fistulated cow and straining this through three layers of cheesecloth, pressing the residue by hand directly into a glass container in which the air had previously been replaced by nitrogen or carbon dioxide. To minimize heat losses, the receptacle was wrapped in paper. After thorough mixing, 700-ml aliquots of this liquor were incubated with 12.0 (and later 20.0) g of Whatman #12 filter paper (ground in a Wiley mill to pass the 0.5 mm sieve), 15.0 ml of 1 M KH_2PO_4, and 7.5 ml each of 2 M $(NH_4)_2\ SO_4$, 1 M Mg SO_4 and 0.5 M $CaCl_2$ in a glass bottle and in a semipermeable bag, using the type of apparatus presented in Fig. 1. Two fermentations were conducted at one time in duplicate apparatus, differing only in vessel *B*, which in one was a glass bottle, and in the other a "Visking" cellulose sausage casing of 4½ in. diam. In both, vessel *B* is closed by *a*, which is a wood stopper surrounded by a rubber gasket and coated with wax, and suspended in a 12 × 12-in. water bath *H*. Tubes *c* and *d* serve as inlets and outlets for gas, respectively; tube *e* is connected to a burette containing 0.5 M Na_2CO_3; *g* is a mercury-sealed shaft of an adjustable speed stirrer;

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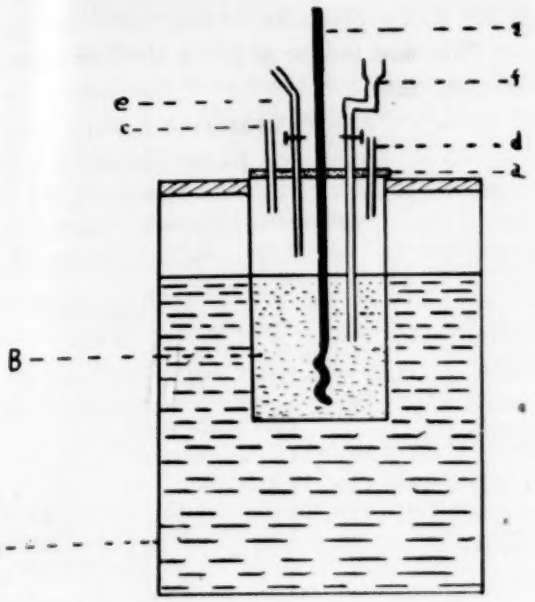

FIG. 1. Apparatus for study of rumen digestion *in vitro*.

and *f* is a tube fitted with a stopcock and a reservoir into which fluid can be drawn for pH determinations. The temperature of the water baths was thermostatically controlled to a temperature of 39° C. After Experiment 1, a current of nitrogen gas replaced the motor-driven stirrer in the bath carrying the semipermeable bag. This ensured adequate mixing as well as improved anaerobic conditions.

Prior to commencing an experiment, tube *c* was pushed well down into the fluids in *B* and a rapid current of nitrogen containing 5% carbon dioxide passed through for about 3 min, after which the tube was raised above the surface of the fluid and the gas flow maintained at a low rate for the duration of the experiment. The pH of the fluids in *B* was then adjusted to 6.8, by running in alkali through *e* while stirring. The inorganic salt concentrations and pH were adjusted in the water bath to the levels inside the semipermeable bag, and the fermentation allowed to proceed at 39° C in both the glass bottle and in the semipermeable bag for a period of 24 hr at or near a pH of 6.8.

The frequency and amount of stirring to which the fermenting fluids were subjected varied with the different experiments, as indicated in Table 1. The pH was adjusted to about 6.8 at 2-hr intervals. As the pH seldom dropped below 6.3 in the course of 2 hr, more frequent adjustments were deemed unnecessary.

TABLE 1

In Vitro DIGESTION OF CELLULOSE

Experiment	Semipermeable bag			Glass bottle		
	Wt in g of cellulose			Wt in g of cellulose		
	At start	Residue	Digested	At start	Residue	Digested
1*	11.89	5.00	*6.89*	11.89	6.91	*4.98*
2*	11.58	3.77	*7.81*	11.58	6.56	*5.02*
3†	19.07	3.42	*15.65*	19.07	10.26	*8.81*
4‡	19.36	6.10	*13.26*	19.36	8.12	*11.24*

* Stirred for 1 min at 1-hr intervals.
† Stirred for 1 min at ½-hr intervals.
‡ Stirring in glass bottle the same as in Experiment 3; stirring in the bag for 3 min at ½-hr and for 10 min at 2-hr intervals.

At the end of 24 hr, the "brews" were made up to definite volumes, well shaken, and aliquots, usually 50 or 100 ml, taken for the determination of residual cellulose using the method of Crampton and Maynard (*1*). The cellulose content of the rumen liquor before incubation was also estimated.

The production of volatile fatty acids was determined in the final experiment, 4. The volatile acids in the steam distillates were transferred to 25 ml of chloroform containing 10% butanol (CB_{10}), according to a method followed by Elsden (*2*). Total acids were then titrated with 0.1 *N* $Ba(OH)_2$ solution, using phenol red as indicator. Finally, 1-ml aliquots of the CB_{10} extracts were used to separate the total volatile acids chromatographically, according to the method of Peterson and Johnson (*4*), into a propionic acid fraction representing the portion eluted by benzene, and an acetic acid fraction representing the portion eluted by CB_{10}.

Some preliminary data on cellulose digestion during 24 hr are presented in Table 1. In comparison, calculations from Marston's (*loc. cit.*) data showed that he obtained cellulose digestion to the extent of 5–7 g.

The variable, so far as could be determined, was the type of container in which the digestions were conducted. Therefore, the differences in favor of the semipermeable bag recorded in Table 1 for all four trials appear to be due only to an inhibitory influence of fission products accumulating in the glass bottle. And if this is so, differences in rates of fermentation may be expected to be greater, the higher the activity of the particular sample of rumen liquor incubated. This is borne out by the results of Experiments 1–3. Precisely what led to these differences in activity in samples of rumen fluid collected, admittedly on different dates, but at the same time of day from the same animal on the same dietary regime, it is not possible to say at present. Mention must be made, however, of the probability that the ideal, according to which the transfer of the rumen liquor from the living animal to the artificial environment should be undertaken with the least delay, exposure to air, and loss of heat, was progressively approached from Experiments 1–3.

For reasons not connected with this work the ration of the cow was changed about a week prior to Experiment 4, from hay plus a concentrate to hay only. Judged by the frequency of certain characteristic churning movements set up periodically in the fermenting liquors by gas accumulating in the cellulose settling gradually to the bottom of the containers and then escaping to the surface, the rumen sample obtained after this change of diet was particularly active. This conclusion was supported by the comparatively high rate of cellulose breakdown registered for the glass bottle in Experiment 4. The rate of fermentation in the semipermeable bag again exceeded that in the glass bottle but not to the extent that might have been expected from results of Experiment 3. An explanation may be the amount of stirring used, as noted in Table 1. The higher rate of stirring applied to the semipermeable bag may have had an adverse effect, in

view of Marston's observation that rapid and frequent stirring caused decreased fermentation.

Results of the analysis of the fermentation products obtained in Experiment 4 are presented in Table 2. These data indicate that: (1) acetic and propionic acids are

TABLE 2

VOLATILE ACIDS IN "BREWS" AND DIALYZATE AFTER 24-HR FERMENTATION (G)

Acid	Introduced with 700 ml rumen liquor	Glass bottle	Semipermeable bag		
			Inside	Outside	Total
Propionic ..	1.56	5.11	2.58	2.64	5.22
Acetic	2.26	5.28	2.22	2.38	5.60
Total	3.82	10.39	4.80	6.02	10.82

formed in the same proportions, irrespective of whether fermentation is carried out in the glass or cellophane container; (2) apparently less total volatile acid/g of cellulose digested is formed in the semipermeable bag than in the glass bottle, and (3) acetic acid diffuses at a greater rate from the bag than does propionic acid.

In conclusion it may be mentioned that there was no microscopic evidence of attack by the ruminal organisms on the cellophane bag. This is indeed fortunate, and may be explained on the basis that attack on cellulosic materials usually begins at broken or torn ends and no such surfaces were exposed in these experiments.

This work is being continued to explore ways of improving the technique.

References

1. CRAMPTON, E. W. and MAYNARD, L. A. *J. Nutrition*, 1938, **15**, 383.
2. ELSDEN, S. R. *Biochem. J.*, 1946, **40**, 252.
3. MARSTON, H. R. *Biochem. J.*, 1948, **42**, 564.
4. PETERSON, M. H. and JOHNSON, M. J. *J. biol. Chem.*, 1948, **174**, 775.

A Simple Universal Dairy Products Phosphatase Test

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Dairy products vary quite widely in character and composition. For example, there is the extreme contrast that exists between whole milk and ripened cheese. This variability has affected the applicability of standard phosphatase methods to various dairy products. Until recently, the Kay-Graham phosphatase test could not be used for ripened cheese or for chocolate milk products because interfering substances in them turned blue with the Folin-Ciocalteu reagent, resulting in false positive readings.

In addition, in all present-day phosphatase tests, including Sanders and Sager's (*3*), the concentration of the buffer and precipitating agent has to be shifted for many of the dairy products having different buffer capacities. This has led to a great deal of added effort and much confusion.

TABLE 1

SENSITIVITY OF THE CORNELL PHOSPHATASE METHOD WITH FRESH WHOLE MILK AND 15-MONTH-OLD CHEDDAR CHEESE

Milk	Phenol γ/0.5 ml*	Cheddar cheese	Phenol γ/0.25 g*
Pasteurized†	1.3	Made from	
" + 0.1% raw	14.2	pasteurized	
" + 0.4% raw	47.0	milk	
" + 1.0% raw	64.0	" + 0.1% raw	8.2
		" + 0.4% raw	22.0
		" + 1.0% raw	30.0

* Tentatively, any value over 5.0 γ phenol/0.5 ml milk or 0.25 g cheese is considered underpasteurized.

† Pasteurized milk for this test is milk heated to at least 143° F for 30 min.

During the past year, an ether extraction technique has been evolved for the elimination of all interfering substances in dairy products when using the Kay-Graham phosphatase method with the Folin-Ciocalteu reagent (*1*). This work also brought out the fact that the barbital buffer used in the Kay-Graham test was unsatisfactory. As a result, a highly concentrated sodium carbonate-sodium bicarbonate buffer was substituted, with excellent results (*2*).

Though the extraction process proved highly satisfactory in improving the Kay-Graham test, information obtained during this investigation pointed toward the development of a very simple procedure for all dairy products without extraction when using 2,6-dibromoquinonechlorimide (BQC) as the color indicator.

The method used in this new test is to incubate dairy product samples with a sodium carbonate-sodium bicarbonate buffer substrate at pH 9.5–9.7 for a period of time, then to precipitate the protein with an acid precipitant. After filtration, the filtrate is brought back to an alkaline reaction with carbonate, and BQC is then added. Blue color is produced if sufficient phosphatase was present in the milk during incubation to split the added disodium phenyl phosphate in the buffer substrate.

This test differs from others in that (1) the buffer and precipitating agent are of different nature, (2) only one concentration of buffer and precipitating solutions is required for all dairy products, and (3) it is not necessary to heat-inactivate the phosphatase enzyme after incubation, since the precipitating agent reduces the milk-substrate solution to pH 1–2.

The details of the method are briefly outlined as follows: 1 ml of milk or fluid dairy product or 0.5 g of cheese or other solid dairy product is mixed with 10 ml of warm carbonate buffer substrate (sodium carbonate 11.5 g; sodium bicarbonate 10.5 g; disodium phenyl phosphate 1.09 g/l).

The test tubes are then incubated at 32–37° C for 18–24 hr. After incubation, 1 ml of an acid precipitant

solution (25.0% trichloracetic acid + 18% hydrochloric acid) is added to each tube. The resulting precipitate is filtered off through Whatman No. 42 paper (11 cm).

Five ml of the filtrate is pipetted into a small test tube, followed by the addition of 1 ml of 0.05% copper sulfate plus 2% Calgon solution, and 5 ml of 8% sodium carbonate solution. Then 2 drops of BQC solution (50 mg/10 ml absolute methyl alcohol) are added, and the colors allowed to develop for 15 min at 37° C. The developed colors are compared with suitable standards.

For ripened cheese, the color development procedure is the same, except that 1 ml of 10% Calgon solution is substituted for 1 ml of the copper sulfate-Calgon solution.

Tentatively, for the long incubation method any value over 5 γ phenol/0.5 ml milk or 0.25 g cheese indicates underpasteurization or raw milk products. A 1-hr test using the identical steps but with a different critical standard has also been developed.

The sensitivity and accuracy of this test are very high, and the value of blanks is low. Table 1 shows some data obtained with milk and aged Cheddar cheese.

References

1. Kosikowsky, F. V. and Dahlberg, A. C. *J. dairy Sci.*, 1948, **31**, 561.
2. *Ibid.*, 1949, **32**, 751.
3. Sanders, G. P. and Sager, O. S. *J. dairy Sci.*, 1947, **31**, 909.

Comments and Communications

Electronic Enhancement of X-Ray Film Contrast[1]

A viewing device for x-ray films, which makes use of some principles of television, promises to be of aid in certain long-standing problems of roentgenology. Many films contain questionable faint shadows whose existence and outlines ought to be ascertained definitely. If such a film is scanned by a densitometer, small but distinct differences in density can sometimes be found in homogeneous-looking regions of the film.

A television camera, using an orthicon tube or other scanning device, is employed as a continuously acting densitometer. The electrical output from the camera is amplified, "clipped" as described here, and fed into a kinescope, or viewing tube, as in conventional television practice.

The new feature of this method lies in the clipping. Unlike photographic film, which necessarily has black as the origin of its intensity-axis, a camera-amplifier-clipper combination can be arranged to ignore all light of less than a prescribed intensity. This is possible because a vacuum tube can be made insensitive to voltages more negative than a chosen threshold. The tube can also have an arbitrarily placed upper limit beyond which it fails to respond. Thus the voltages representing the darkest and the lightest portions of the film are clipped off, permitting those parts of the signal between the limits to be amplified as desired.

Thus a suitably high-gain video amplifier with adjustable cut-offs for both the black and the white ends of the signal can expand any portion of the gray to the full contrast of which the kinescope is capable. Everything darker than a certain gray is reproduced as black and everything brighter than another (lighter) gray comes through as white, but the outlines of those areas which have intermediate brilliance but small contrast are vividly portrayed by the picture tube.

Successive parts of the black-gray-white scale can be examined by rotating a knob, analogous to the brilliance control of a television receiver. Let us imagine a solid, formed by erecting a line perpendicular to the film at each point of its surface, each line having a length proportional to the density of its foot. This solid is cut by two planes parallel to the film (one plane representing black on the kinescope and the other white), and the frustum can be examined in detail. The brilliance control regulates the distance of these planes from the base, and the contrast (sensitivity) control adjusts their distance from each other. The result can be metaphorically described as taking serial sections along the density axis of the film.

To date, a few x-ray films have been viewed by this method, and the improvement in contrast is striking, especially when one personally operates the controls and watches the shifts in emphasis. If the kinescope is photographed with the controls properly set, a more vivid picture results than can be taken from the film directly.

John S. Garvin and Craig W. Goodwin
Illinois Psychiatric Institute,
University of Illinois

[1] The authors wish to express their thanks to the Balaban and Katz Television Studios, Station WBKB, Chicago, for making available equipment for these experiments.

The High School Biology Teacher

Victor A. Greulach (*Science* 1949, **109**, 385) in sharing Dr. Van Overbeek's concern over the inadequacy of high school biology teaching, (*Science* 1949, **109**, 210) exhorts "professional biologists through their societies to support a program designed to improve the quality and quantity of secondary school biology."

A practical and more immediately feasible suggestion might be for some university departments of biology to remove—or at least to open—the academic curtain that they keep tightly drawn between themselves and departments of education. I know of one large university in the East in which the zoology department schedules its courses at hours which seem deliberately designed to keep out high school biology teachers.

It is gratifying to learn of the preoccupation of at least some professional biologists with improvement of the biology literacy of the American public. National, state, and local associations of high school biology teachers are equally concerned and they would welcome the cooperation of their colleagues in colleges, universities, and research institutions.

ZACHARIAH SUBARSKY

Department of Biology,
The Bronx High School of Science, New York

Correction

In my report on the Echo Lakes Symposium on Cosmic Rays (*Science*, September 2, 1949) the π- and μ-symbols were interchanged, in the line before last of the second column, p. 242, and in the first line of the third column in the same page. The portion of the text containing these two lines should read: "... the latest values for the masses of the μ- and the π-meson (215 and 285 electron masses respectively) and for the mean life of the π-meson (0.63×10^{-8} sec)." In a recent letter to the writer, Dr. Barkas states that this value for the mean life of the π-meson has been superseded by more accurate measurements. These measurements, made on positive π-mesons, give a mean life of 1.97×10^{-8} sec. Dr. Barkas also indicates that new mass measurements give 276 electron masses for positive or negative π-mesons and 210 electron masses for positive μ-mesons.

BRUNO ROSSI

Massachusetts Institute of Technology,
Cambridge, Massachusetts

Psilotum Gametophytes Matured under Greenhouse Conditions from Self-sown Spores

Recently, young sporophytic plants of *Psilotum nudum* (L.) Griseb. (Eames, A. J. *Morphology of vascular plants, lower groups.* New York: McGraw-Hill, 1936) were found growing in the pot of a ten-year old *Cibotium* plant and in the soil around a dead *Adiantum* plant taken from the former pot. Mature sporophytic plants of *Psilotum* have been growing for 10 to 12 years in the vicinity of the *Cibotium* plant in the Cornell University Conservatory.

Numerous whole or fragmentary pieces of gametophytes (Darnell-Smith, P. A. *Trans. Roy. Soc. Edinburgh*, 1917, **52**, 79; Holloway, J. E. *Ann. Bot.*, 1939, **3**, 313; Lawson, A. A. *Trans. Roy. Soc. Edinburgh*, 1917, **52**, 93) were found in the soil supporting the sporelings, but none were found in the larger pot where they probably first appeared. Gemmae were observed arising from both the gametophytes and the sporophytic rhizomes as described by Holloway. Antheridia were fairly abundant, and the characteristic four-rowed archegonial necks, both entire and decapitated, were observed on nearly all of the gametophytes. Structures which could unquestionably be called embryos were not seen. During the examination of the gametophytes in water under a dissecting microscope, the heat from the electric light bulb warmed the water sufficiently to cause the antheridia to open apically to discharge the sperms. Under the artificial conditions, the sperms were short-lived and died within an hour or less if they did not come in contact with archegonia. The striking, coiled, hyaline sperms were not seen to enter the archegonia, but apparently to lie down, as it were, and remain across the open end of the decapitated necks.

This brief description of a possible source of gametophytes of a plant so important to morphologists and systematists seems worthy of note, for, insofar as the authors are aware, the gametophytes of *Psilotum* have not before been cultured to maturity, either intentionally or, as here, unintentionally, and such culture is considered impossible by many. Lawson germinated spores under nearly natural conditions but it is not clear from his report whether he grew the gametophytes to maturity or not.

M. F. MOSELEY, JR., and
BESSIE C. ZIMMERLY

Santa Barbara College,
University of California,
Santa Barbara, California and
Department of Botany,
Cornell University, Ithaca, New York

Rainfall of Fish

With regard to the communication (*Science*, 1949, 109, 402) concerning the rainfall of fish, another pertinent experience may be of interest.

While stationed on the island of Guam in September, 1936, I witnessed a brief rainfall of fish, one of the specimens of which was identified as the tench (*Tinca tinca*) which, to my knowledge, is common only to the fresh waters of Europe. The presence of this species at a locale so remote from its normal habitat is worthy of note.

J. HEDGEPATH, MAJOR, U. S. ARMY (ret.)

General Delivery,
Aberdeen, Maryland

Book Reviews

Unitary principle in physics and biology. Lancelot Law Whyte. New York (10): Henry Holt, 1949. Pp. xxiii + 162. $3.50.

The development of fundamental sciences during the first half of the present century was characterized on one hand by clarification of basic principles underlying physical phenomena (relativity, uncertainty, elementary particles), and on the other by much deeper insight into basic phenomena from the realm of biology (interpretation of genes and viruses as single molecules, etc.) which gives us promise of understanding the phenomena of life on a purely physical basis. This led recently to a much closer collaboration between physicists and biologists, and induced many theoretical physicists (for example, E. Schroedinger) to attempt an extension of the well-developed methods of physical theory into the virgin field of fundamental biology. Although, as could well be expected, the development of the theory of organic matter still represents tremendous difficulties, one can hope that further experimental studies of elementary biological phenomena, and more extensive work on their interpretation in the language of molecular physics, will lead in the future (maybe not so distant) to the complete solution of the ancient riddle of life.

That is why one witnesses with a deep chagrin the appearance of such books as one by Lancelot Law Whyte who claims to be able to turn upside down both physics and biology, and to arrange them into new order on the basis of a newly invented philosophical (in the bad sense of this word) principle of "unitarity."

It is hardly necessary to discuss in any details the contents of this book, which will be quite unacceptable to all physicists (even to those possessing a great sense of humor), and of little help to any biologist. Thus we will give only a few quotations typical for the entire presentation. The "Unitary Principle" from which all laws of physics and biology are to be derived reads:

"Asymmetry tends to disappear, and this tendency is realized in isolable processes." In applying this principle to physical phenomena, the author comes to a number of conclusions which, according to him "*offer a challenge to the theoretical physicists* either to justify them by quantitative confirmation, or to show that they contradict established facts." Here are two examples of such conclusions: "Atom is a stable pattern with one center of (latent) symmetry." "Electronic current (at temperatures not close to absolute zero) is a continued induction, by a chemical source of polarization, of an extended state of polarization (in a conductor) which continually collapses into disordered molecular polarization."

Having revolutionized physics by these and other similar statements, the author turns to biology and defines, for example, a green plant as: ". . . a continuous process of local chirol fields stabilized by hereditary units and supported by stationary structures conforming to an external axis."

Not being a botanist, the reviewer is unable to judge the merits of this definition.

The quotations are sufficient commentary on this book.

GEORGE GAMOW

George Washington University

Factors of evolution: The theory of stabilizing selection. I. I. Schmalhausen. (Theodosius Dobzhansky, Ed.; I. Dordick, Trans.) Philadelphia: Blakiston, 1949. Pp. xiv + 327. (Illustrated.) $6.00.

Although its dust jacket calls *Factors of evolution* the "first full discussion of new contributions to evolution in more than a decade," it fails to show any advantage of its recency. It was written six years ago, but the bibliography does not cite any non-Russian papers after 1940, and only a few Russian ones. However, a large body of data from the USSR is revealed, and most biologists should find a number of interesting notes on work in their own field.

This book is directed toward establishing the separation of natural selection into a dynamic role and a stabilizing one. Those factors which tend to change the average phenotype of a population are considered dynamic, while those which tend to preserve the population within a narrow range of variation are considered stabilizing. Schmalhausen focuses attention on that group of factors which have the effect of stabilizing the genetic and morphogenetic systems of organisms, and which, together with the elimination of harmful mutants, form the basis of stabilizing selection.

A broad background of work in genetics is used as an introduction to the book, followed by a discussion of natural selection and its consequences, and then a section on the evolution of morphogenetic reactions. The concluding section consists of a discussion of the factors involved in rates of evolution. In each case the facts are selected to fit into the general picture of stabilizing selection, little attempt being made to present a discussion of the whole field of evolution.

The main value of this book will undoubtedly be in the intensive discussion and work it will generate. This is because the number of interesting ideas presented is large, but often the evidence for them is sufficiently meager or the interpretation sufficiently questionable to warrant much further investigation. The publisher's suggestion for using the book as a text, or its use for any general purpose, should find little acceptance. The language is difficult and often obscure. Readers with a general knowledge of taxonomy or genetics will discover many errors and inconsistencies.

It should be noted that Schmalhausen's ideas come from within the framework of Mendelian genetics. Throughout the book the discussion of the roles of en-

vironment and heredity in development are distinctly stated without prejudicing either one. However, the advocacy of Mendelism in this book cost Schmalhausen his position in August of 1948.

HERMAN SLATIS

University of Chicago

From Euclid to Eddington: A study of conceptions of the external world. (The Tarner Lectures, 1947.) Edmund Whittaker. New York: Cambridge Univ. Press, 1949. Pp. ix + 212. $4.00.

Although the title suggests a systematic history of cosmological speculations, and the subtitle hints at a philosophy of science, the present book (containing the substance of the Tarner Lectures for 1947) is in fact neither. Its aim is to outline the development of some of the chief theoretical principles of modern physics, especially those which contribute to the systematic unification of physical knowledge and have a bearing on larger cosmological questions. The volume is thus addressed to readers concerned with the state of current theory; and Whittaker employs historical materials primarily to introduce modern notions and to make evident what is novel in them. His exposition is divided into five parts, dealing in sequence with basic ideas in the analysis of space and time, with the fundamental concepts of classical physics, with the notions underlying general relativity, with the concepts of quantum theory, and finally with Eddington's cosmological views.

Whittaker writes with his customary clarity, though he does assume more than mere literacy on the part of his audience; and he makes excellent use of his unusual mastery of the history of science to illuminate recent attempts at a unified conception of the physical world by showing the relevance of earlier efforts in this direction. Moreover, he expounds the remarkable achievements of modern theoretical physics with contagious admiration and enthusiasm; and his reasonably intelligible account of Eddington's fascinating but not widely known contributions to physical cosmology constitutes a specially useful service to the general reader.

However, the book has little to offer to anyone interested in the philosophical and logical analysis of modern theoretical formulations. Whittaker is indeed often on the verge of interesting methodological reflections, as in his brief comment on electrons as not being "particles in the old sense," or in his over-all remark on the use of alternative models in interpreting the formalism of quantum theory. He also tantalizes his readers by such asides as that recent work on the unified field theory, while marking important contributions to pure mathematics, does not possess much physical significance. It is also evident that Whittaker does not subscribe to the positivist-pragmatist account of the function of physical theory, as is clear from his somewhat negative evalution of Newtonian gravitational theory because of the latter's silence on the origin and mode of transmission of gravitational influences. But none of these essentially incidental observations are adequately developed, and in consequence the reader is never permitted to inspect the intellectual center from which Whittaker surveys and evaluates the fundamental ideas of modern physics.

ERNEST NAGEL

Columbia University

Atmospheric electricity. J. Alan Chalmers. New York (11): Oxford Univ. Press; Oxford, Engl: Clarendon Press, 1949. Pp. 175. (Illustrated.) $3.75.

The appearance of a new book on atmospheric electricity is most welcome. The textbooks of Chauveau, Mache and Schweidler, Benndorf, Benndorf and Hess, *et al.*, are all more or less obsolete and for English readers only the short monograph of Schonland (1932) and three chapters in Fleming's *Terrestrial magnetism* (1939) by Gish, Torreson, and Schonland were available until now.

Chalmers' book is well written and up to date. One can immediately see that the author has worked a great deal (with Whipple) in this field.

It is especially commendable that the new ideas on the mean lifetime of ions and the equilibrium between small and large ions (Nolan, Whipple, Schweidler) are fully presented and that the thunderstorm phenomena are described in modern form, taking into account the results of the altielectrograph recordings.

The author mentions in the preface that he purposely omitted a chapter on cosmic rays (as, for instance, given in Schonland's book) and I agree with him that a full presentation of these phenomena is not necessary for the explanation of atmospheric-electric phenomena. I wish, however, that the importance of ionization by cosmic rays had been stressed in the discussion of the ionization balance of the lower atmosphere, especially over the oceans. Also, I think, the treatment of the main problem of atmospheric electricity (maintenance of the earth's negative charge), although excellent, is perhaps incomplete, since the reader does not get any information with regard to the transfer of charges from the upper part of the thunderstorm regions to the ionosphere.

I also should have wished to see a quantitative treatment of the role of the various ionizing agencies in the atmosphere: contribution of the alpha, beta and gamma rays from the radioactive products in the atmosphere, their distribution with altitude (W. Schmidt's theory of mass exchange) and contribution of ionization by cosmic rays in the free atmosphere in the troposphere and the stratosphere.

In all other respects, Chalmers' book is a very fine, modern text, which deserves full recognition as a standard monograph and wide distribution among geophysicists and specialists in the field of atmospheric electricity.

VICTOR F. HESS

Fordham University

(*Continued from page 469.*)

Brucella organisms. Werner Braun (Camp Detrick) reported on the variation of *Brucella* and on a selective factor present in the serum of normal *Brucella*-susceptible species which is able to suppress the establishment of avirulent variants. This factor is absent from the serum of relatively insusceptible species, as well as from the serum of infected or vaccinated individuals of susceptible species. It is hoped to develop a new method of therapy based on the inactivation of this "SS" factor *in vivo*. In his discussion of the physiology of *Brucella*, B. H. Hoyer (National Institutes of Health) reported that the urease activity of *Br. abortus* strains is much lower than that of *Br. melitensis* or *Br. suis* strains, and that the urease test, used quantitatively, represents an additional aid in differentiating *Brucella* cultures. Sanford Elberg (University of California) discussed the relatively low resistance of large and small animals to reinfection with *Brucella* organisms, particularly in respect to tissue response, and the dissemination of the organisms in the internal organs of immune animals as compared with susceptible animals.

The evening session of the first day was devoted to a discussion of clinical problems and an evaluation of available chemotherapeutic materials. W. W. Spink (University of Minnesota) reported on his experience over a period of 12 years with over 250 culturally proved cases of brucellosis. He emphasized that an unequivocal diagnosis of brucellosis is established only when *Brucella* organisms are cultured from the tissues or body fluids. Positive agglutination tests are helpful in diagnosing the disease, although skin tests and opsonocytophagic tests are of only doubtful value. In his evaluation of the effectiveness of currently available chemotherapeutic agents, Wesley Eisele (University of Chicago) took the point of view that no satisfactory drug was yet available for the treatment of brucellosis and that many of the reported successes in therapy represented spontaneous recoveries. Dr. Eisele admitted that streptomycin plus sulfadiazine, aureomycin, and some other agents often, or usually, suppressed the disease, but considered that relapse of the infection could be anticipated with great frequency after treatment with such agents.

In opening the morning session of the second day, M. R. Irwin (University of Wisconsin) reported on the presence of bactericidins, particularly for *Br. abortus*, in the blood of experimentally infected or vaccinated cattle, as well as in some normal animals, these antibodies being distinct from agglutinins. A. I. Braude (University of Minnesota), in discussing the pathology of brucellosis, pointed out that the disease in experimental animals is characterized by the formation of granulomas in the tissues, by the development of hypersensitivity, and by the appearance of the *Brucella* organisms within the macrophages. The limitations and interpretations of the common laboratory tests used in diagnosing brucellosis were described by N. B. McCullough (University of Chicago and National Institutes of Health). The use of the guinea pig for chemotherapeutic tests was reported on by B. N. Carle (National Institutes of Health). F. M. Anderson (Federalsburg, Maryland) reviewed an outbreak of brucellosis in the small town of Federalsburg, Maryland, in closing the morning session.

The afternoon session of the second day was devoted to the epidemiology of brucellosis. C. F. Jordan (Tarrant County, Texas, Health Department) reported that, for the seven-year period 1940–1946, the average yearly aggregate of reported cases of brucellosis in the United States was at the rate of 3.11 per 100,000 population. *Br. abortus* infections predominate in areas devoted to dairying or cattle-raising, whereas *Br. suis* infections are most common in the meat-packing industry and in hog-raising areas. S. R. Damon (Indiana State Health Department) presented the results of studies on the epidemiology of brucellosis in Indiana, a much lower incidence of the infection being encountered than was considered probable when the study was initiated. P. Morales Otero (Santurce, Puerto Rico) reported that brucellosis was first discovered in 1921 in Puerto Rico, and that, in the years since, the infection had become disseminated in cattle throughout the island. The incidence of the infection in man has increased correspondingly and is currently five times that reported in the island in 1930. During 1948, the Insular Government inaugurated a brucellosis eradication program in Puerto Rico. The incidence of brucellosis in Canada varies greatly among different provinces, according to R. Gwatkin (Canadian Department of Health). Steps are being taken by the Canadian Government to aid in the control of the disease among cattle and swine. R. Newton (Swift and Company) briefly outlined the problems which the meat-packing industry faces in brucellosis. In closing the meeting, C. M. Carpenter (University of California at Los Angeles) summarized his experiences in studying brucellosis over approximately twenty-five years, particularly in respect to the use and interpretation of skin test responses to *Brucella* antigen.

Association Affairs

THE DECEMBER MEETING

All arrangements for the 116th Meeting of the AAAS, December 26–31, are proceeding on schedule despite indications that this will be the largest annual convention in the 101-year history of the AAAS. All but one of the Association's 17 sections and subsections and nearly 60 participating societies will have programs—most of them with multiple sessions. The five Penn Zone hotels and the Manhattan Center (with some sessions on the campus of Columbia University) make it possible to maintain the integrity of each society's sessions and yet retain the advantages of a large diversified meeting in one compact area within a radius of three city blocks.

Ten symposia sponsored by the Association are for the most part grouped on Thursday and Friday, December 29 and 30. A summary of all the programs and a description of the 70 exhibits that fill the 85 booths—all that could be made available this year—will appear in the preconvention issue of *Science*, December 9, 1949. Advance registrants will receive the general program, of over 300 pages, the first week in December.

It would be impossible to make arrangements for the large and complex meetings of the AAAS, to ensure their smooth operation, and to secure adequate publicity for the programs and special events without the cooperation of a great many individuals and the generous contribution of their time and effort. This year, the 70-odd section and society secretaries (with few exceptions) were prompt in sending in their copy, including more than 2,000 titles; the printer of the general program has maintained a tight schedule for galley and page proof, and the AAAS local committees already have begun their substantial duties. A complete list of these committees follows:

General Chairman

Joseph W. Barker, president, Research Corporation.

Executive Committee

(Composed of the chairmen of the six local committees)

Joseph W. Barker, chairman; *George S. Avery, Jr.*; *Harold H. Clum*; *Stuart C. Dorman*; *Robert Harron*; *R. W. King*; *George B. Pegram.*

Advisory Committee

George B. Pegram, vice president, Columbia University, chairman; *Frank G. Boudreau*, executive director, Milbank Memorial Fund; *Donald E. Lancefield*, Queens College; *Albert E. Parr*, director, American Museum of Natural History; *William J. Robbins*, director, New York Botanical Garden; *Sam F. Trelease*, Columbia University.

Equipment and Projection Committee

Harold H. Clum, Hunter College, chairman; *H. G. Albaum*, Brooklyn College; *P. L. Bailey*, The City College; *C. A. Berger*, S.J., Fordham University; *Richard B. Conklin*, Brooklyn College; *Gordon G. Darkenwald*, Hunter College; *J. A. Dawson*, The City College; *Ray F. Dawson*, Columbia University; *William T. Gormley*, Hunter College; *Louis Levine*, The City College; *Daniel Ludwig*, New York University; *Walter H. Mais*, Brooklyn College; *Edwin B. Matzke*, Columbia University; *Morris Meister*, principal, Bronx High School of Science; *Arthur W. Pollister*, Columbia University; *Don D. Ritchie*, Barnard College; *R. W. Root*, The City College; *Leonard P. Sayles*, The City College; *Simon Share*, Queens College; *Arthur Sweeny, Jr.*, Hunter College; *Edward M. Thorndike*, Queens College; *George S. Tulloch*, Brooklyn College; *Morris Winokur*, The City College; *Bertha Wittlinger*, Jamaica High School; *Wilson W. Woodcock*, Hunter College; *Donald D. Wright*, Brooklyn College; *Fred Zaher*, Brooklyn College.

Finance Committee

R. W. King, Bell Telephone Laboratories, chairman; *J. C. Boyce*, Department of Physics, New York University; *C. L. Brown*, Standard Oil Development Company; *H. M. Chadwell*, Atomic Energy Commission; *Richard Courant*, Institute for Mathematics and Mechanics, New York University; *L. M. Currie*, National Carbon Company, Inc.; *R. J. Dubos*, Rockefeller Institute for Medical Research; *C. G. King*, Nutrition Foundation; *R. E. Kirk*, Polytechnic Institute of Brooklyn; *R. C. Swain*, American Cyanamid Company.

Publicity Committee

Robert Harron, Director of Public Information, Columbia University, chairman; *Lawrence W. Bass*, vice president, U. S. Industrial Chemicals, Inc.; *Martin J. Freeman*, director of public relations, Hunter College; *Richard S. Henry*, Office of Public Information, Columbia University; *Israel E. Levine*, assistant director of public relations, The City College; *Edward B. Lyman*, assistant to the president, Fordham University; *Henry S. Miller*, director of public relations, Queens College; *Robert L. Taylor*, editor, *Chemical Industries*; Louise Warner, publicity director, Polytechnic Institute of Brooklyn; *Hayden Weller*, director of press relations, New York University, vice chairman.

Reception and Entertainment Committee

George S. Avery, Jr., director, Brooklyn Botanic Garden, chairman; *Ralph A. Beals*, director, The New York Public Library; *W. Tresper Clarke*, chief chemist, Rockwood and Company; *John G. Phillips*, president, International Business Machines Corporation; *George L. McNew*, director, Boyce Thompson Institute for Plant Research; *J. R. Poppele*, vice president and chief engineer, WOR; *Allen Porter*, secretary, Museum of Modern Art; *Leander Query*, public relations manager, New York Tele-

Phone Company; *Floyd D. Rogers, Jr.*, manager, public relations, The Metropolitan Museum of Art; *Hardinge Scholle*, director, Museum of the City of New York; *John Tee-Van*, executive secretary, New York Zoological Society; *F. E. Vilas*, Consolidated Edison Company of New York.

Reference Committee

Stuart C. Dorman, manager, College Department, McGraw-Hill Book Company, chairman; *Mahlon Ashford*, New York Academy of Medicine; *Robert S. Mathews*, Bennett and Pleasant; *Ross F. Nigrelli*, New York Aquarium; *Elliott R. Weyer*, vice president, Pfizer Chemical Company.

The 116th Meeting of the AAAS, and the annual meeting for the year 1949, is also the sixth New York meeting. The previous occasions when the Association met in New York City were 1887, 1900, 1906, 1916 and 1928. It will be noted that an interval of 21 years has elapsed since the fifth New York meeting, when Henry Fairfield Osborn and Arthur A. Noyes were president and retiring president, respectively.

The organization of the meetings is much the same, a number of the exhibitors in the 1949 Annual Science Exposition were present in 1928, the roster of the participating societies retains many of the same names—and there will be a gratifying number at the sessions of 1949 that also were there 21 years ago. George B. Pegram, of Columbia, who was general chairman of local committees in 1928; Sam F. Trelease, who edited the program that year; and Donald E. Lancefield, then chairman of the special committee on local transportation, are all serving on the Local Advisory Committee this year. Edmund W. Sinnott, of the 1928 Local Committee on Exhibitions, is the 1949 retiring president of the Association, while Roger Adams, current president elect, was chairman of the AAAS Committee on Prize Awards at the fifth New York meeting. Once again, the Biologists' Smoker will be held at the American Museum of Natural History.

But there are differences. A whole generation of young scientists has reached maturity and increased the membership of the Association. This year's meeting is larger in participating organizations and includes new societies, the papers are more numerous, the attendance is greater, and there are more exhibitors. The greatest differences, however, are the advances that have been made in science itself, the technological innovations and their effects on man's thinking.

NEWS and Notes

George D. Stoddard, president of the University of Illinois, has been elected chairman of the U. S. National Commission for Unesco. Dr. Stoddard succeeds Milton S. Eisenhower, whose terms of office have reached the limit of three set by the bylaws.

Karl A. Folkers, director of organic and biochemical research for Merck and Company, Inc., will deliver the fifth annual Harrison Howe Lecture at a meeting of the Rochester Section of the American Chemical Society, November 7. Dr. Folkers will speak on "Research on Vitamin B_{12}."

Tze-Tuan Chen, formerly professor of zoology at the State Teachers College at Bemidji, Minnesota, has joined the Department of Zoology staff of the University of Southern California. Dr. Chen will continue his work on protozoan cytogenetics, which has been supported by a Public Health Service grant.

Alma P. Beaver, associate professor of psychology at Santa Barbara College of the University of California, will conduct a survey of educational and clinical methods in the Scandinavian countries from January to June, 1950. Dr. Beaver is on sabbatical leave from the university, and is now making a study of psychological theories and methods of therapy at the Langley Porter Clinic, San Francisco.

Clarence F. Winchester, formerly associate professor of nutrition at the University of Florida, is now physiologist at the Animal Husbandry Division, U. S. Department of Agriculture Research Center, Beltsville, Maryland. Dr. Winchester is in charge of research projects relating endocrinology to problems of nutrition.

Elizabeth A. Lockwood has been appointed as field consultant in health education by the National Foundation for Infantile Paralysis. She will advise professional groups and state representatives of the foundation in the development of health education programs relating to poliomyelitis.

George Howard Parker, professor emeritus of zoology, Harvard University, has been made patron of the Bermuda Biological Station for Research by election of the station's board of trustees.

James F. Kerwin, research chemist, has been named assistant head of the Organic Chemistry Section of Smith, Kline and French Laboratories in Philadelphia.

Visitors to U. S.

Pandit Jawaharlal Nehru, Indian prime minister, and his sister, ambassador to the U. S., conferred in Boston last week with James B. Conant and Harlow Shapley, of Harvard University, about a new Schmidt-type telescope for the Allahabad University Observatory in north India, and a sun-observing coronagraph for Mt. Kodaikanal Solar Observatory in southern India. **A. C. Banerji,** of Allahabad, and **A. K. Das,** of Mt. Kodaikanal, will visit Harvard later to work out the details of the project.

Sven Hellerstrom, professor and chief of the Dermatologic and Venereal Disease Clinic of the University of Stockholm, lectured last week at the New York University-Bellevue Medical Center.

Felisberto Pinto Monteiro, of Rio de Janeiro, is in this country for research in the field of fish and wildlife. Mr. Pinto has received a three months' grant-in-aid from the U. S. Department of State.

Recent visitors at the National Bureau of Standards included: **D. J. Badkas,** assistant professor, Indian Institute of Science, Bangalore, India; **D. H. Black,** director, Telecommunications Research and Development, Ministry of Supply, England; **E. Heymann,** associate professor of physical chemistry, University of Melbourne, Australia; **L. E. Howlett,** associate director, Division of Physics, National Research Council, Ottawa, Canada; **J. S. McPetrie,** superintendent of research, Signals Research and Development Establishment, England; **C. Pot,** general director, Electro-Smit Dynamoworks, Slikkerveer, Holland; **H. N. Ramachandra Rao,** assistant professor, Department of Power Engineering and Electrical Technology, Indian Institute of Science, Bangalore; **Yves Rocard,** chef du Service des Recherches, Direction Centrale des Constructions et Armes Navales, Paris, France; **T. Edward Allibone,** director, Associated Electrical Industries, Ltd., Research Laboratories, Aldermaston, Berkshire, England; **Lucien Biguet,** director, Department of Chemical Products, S. A. Coframet, Paris; **Luis Bramao,** chief, Department of Soils, Portugal; **A. J. Dale,** director of research, Johnson Brothers, Ltd., Stoke-on-Trent, England; and **M. A. Malik,** electrical engineer, Public Works Department, Lahore, Pakistan.

Grants and Awards

The Board of Foreign Scholarships of the State Department has announced the teaching and research awards made for this academic year under the terms of the Fulbright Act. Among the American exchange scholars are: *Bernard Freeman Mann,* resident in pathology, New England Deaconess Hospital, Boston, selected as visiting lecturer in pathology at University of the Philippines; *Fred Eggan,* professor and chairman of the Department of Anthropology, University of Chicago, for anthropological research in the Philippines; *Pauline Fertsch,* assistant professor of psychology, University of Texas, visiting lecturer in psychology at the University of the Philippines; *Ruth M. Leverton,* director of human nutrition and food research, Department of Home Economics, University of Nebraska, to teach nutrition at the Philippine Women's University; *Jesse E. Thompson,* chief resident surgeon, Massachusetts Memorial Hospital, Boston, for research in medicine at Nuffield Medical Research Institute, Oxford University; *Robert L. Driver,* interne, Hospital of the University of Pennsylvania, for research in neuropharmacology and neurophysiology, University of Edinburgh, Scotland; *Randolph W. Chapman,* professor of petrography, Johns Hopkins University, visiting lecturer in geology, St. Andrews University, Scotland; *Elmer B. Tolsted, Jr.,* assistant professor of mathematics, Pomona College, visiting lecturer in mathematics at University College of the Southwest, Exeter, England; *Roger W. Russell,* associate professor of psychology and resident fellow in neurophysiology at University of Pittsburgh and Western State Psychiatric Institute, for research in animal psychology at the Institute of Psychiatry, University of London; *Elwood C. Zimmerman,* associate entomologist, experiment station of the Hawaiian Sugar Planters Association, for research on insects of Hawaii at the British Museum of Natural History; *Darragh E. Nagle,* instructor in physics, Massachusetts Institute of Technology, for research in nuclear physics, Cambridge University; *David E. Schneider,* teaching fellow, Department of Social Relations, Harvard University, lecturer in anthropology, London School of Economics; *Sewell Wright,* professor of zoology, University of Chicago, visiting lecturer in statistical genetics, University of Edinburgh; *Josef F. Bunnett,* assistant professor of chemistry, Reed College, Portland, Oregon, for research in theoretical organic chemistry, University College, London; *Ernest Sachs, Jr.,* senior fellow in neurosurgery, Yale University, research in lobotomy, National Hospital, University of London; *Clyde Stormont,* assistant professor of genetics, University of Wisconsin, for research in dairy science at the Animal Research Station, Wallaceville, New Zealand; *Basil E. Montgomery,* associate professor of entomology, Purdue University, Lafayette, Indiana, for research in the ecology of the bumble bee in relation to the fertilization of red clover, University of New Zealand, Auckland; *William R. Vance,* assistant professor of natural science, Chico State College, Chico, California, visiting professor of public health, University of Rangoon, Burma; *C. Donnell Turner,* chairman, Department of Biology, Utica College of Syracuse University, visiting professor of biology, University of Rangoon; *Roy G. Bose,* professor of psychology, City College, Santa Monica, California, visiting lecturer in child guidance, Philippine Normal School, Manila; *William J. Emerson,* medical investigator, Massachusetts General Hospital, Boston, for research on medicine, University of Paris; *Edgar Bright Wilson, Jr.,* professor of chemistry, Harvard University, for research in chemistry, Queens College, Oxford University; *John Richard King,* postdoctoral student at Harvard University, visiting professor in agriculture, State Training College for Teachers, Rangoon; *Kerro Knox,* research fellow, Brookhaven National Laboratory, for research in chemistry in the Department of Physical Chemistry, Cambridge University; *Edward M. Corson,* senior physicist, Armour Research Foundation, Illinois Institute of Technology, Chicago, for research in physics, Edinburgh University; *John W. Swensson,* formerly physicist at University of California Radiation Laboratory, for research in physics, University of Liège, Belgium; *Harry H. Turney-High,* professor and head of the Department of Anthropology and Sociology, University of South Carolina, Columbia, for research in sociology at University of Liège; *Peter Van de Kamp*

rofessor of astronomy, Swarthmore ollege, director of Sproul Observa- ry, visiting lecturer in astronomy, stitut d'Astrophysique, Paris; *orace M. Miner*, associate profes- r of anthropology and sociology, niversity of Michigan, for research sociology, University of Algiers, lgeria; *Nathan James Smith*, re- earch fellow in hematology, Chil- ren's Hospital, Boston, for research medicine, University of Paris; *uy W. Adriance*, head of the De- artment of Horticulture, Agricul- ural and Mining College of Texas, ollege Station, Texas, for research horticulture, Italy; and *Howard . Etzel*, research physicist, Car- egie Institute of Technology, Pitts- urgh, for research in physics at the aboratoire de Physique, University f Paris.

The J. Lawrence Smith Medal f the National Academy of Sciences as been awarded to Fred Lawrence Whipple, chairman of the Depart- ent of Astronomy of the Harvard Observatory. Dr. Whipple received he award in recognition of his re- earch in the field of the motions and ature of meteoric particles in the arth's atmosphere, and especially or his derivation of the variations f air temperature with altitude, ased on his studies of meteors.

Fellowship

Approximately 600 U. S. Government awards for foreign study are available from the Department of State for graduate study or research during the academic year 1950–51. The countries with which this educational exchange program is now in effect are Belgium and Luxembourg, Burma, France, Greece, Iran, Italy, Netherlands, New Zealand, Norway, the Philippines, and the United Kingdom. Closing date of these competitions is *December 1*. The awards, made available through the Fulbright Act, provide tuition, transportation, and maintenance for one academic year. Candidates for student awards must be American citizens, and must have a college degree or its equivalent, as well as a knowledge of the language of the country in which they intend to study. They should apply to the Institute of International Education, 2 West 45th Street, New York City 19. Candidates at the postdoctorate level, or those not having a doctorate but of recognized professional standing, who propose to teach or carry out an independent program of research abroad, should apply to the Conference Board of Associated Research Councils, 2101 Constitution Avenue, Washington 25, D. C.

Colleges and Universities

University of Michigan scientists have recently obtained samples of the earth's atmosphere at a height well over 40 miles by the use of V-2 and Aerobee rockets. The research is being done by the Department of Aeronautical Engineering as a part of the U. S. Army Signal Corps program of investigation of meteorological phenomena at high altitudes. The project, which is under the direction of Leslie M. Jones, of the university's Engineering Research Institute, is particularly concerned at present with the concentration of helium at the earth's surface and in the upper atmosphere. Preliminary analysis of the samples indicates that the atmosphere up to 230,000 feet has the same relative proportions of gases as at ground level. The gas analysis was done by F. A. Paneth of Durham University, England, who has been investigating the concentration of helium in the atmosphere on a world-wide basis. Results of this research will eventually be used in studies of rocket design and weather.

The University of Wisconsin opened its new Enzyme Institute on November 1, under the direction of David E. Green. The Atomic Energy Commission and the U. S. Chemical Corporation will collaborate with the institute in certain phases of research, and have allocated $30,000 and $20,000, respectively, to the project. The university's Alumni Research Foundation built the $350,000 laboratory and staff salaries are paid by the university.

Meetings and Elections

The Fifth General Assembly of the International Council of Scientific Unions was held in Copenhagen, September 14–16, at the Royal Danish Academy. In welcoming the delegates, the president of the academy spoke of the great value of close cooperation among scientists of different nations and of the need in days of rapidly increasing control of the forces of nature to override the limits imposed by national boundaries.

The main business of the meeting was to consider a revision of the statutes to meet the change of conditions since they were drawn up in 1931. The two chief alterations agreed upon were the formation of a bureau within the executive board, and a classification of the unions into two groups, general and specialized. Of the existing unions the following were classed as general: astronomy, geodesy and geophysics, chemistry, physics, and biology. The remaining unions—radio sciences, geography, crystallography, mechanics, and history of sciences—were classed as specialized. No new unions were accepted as adhering organizations but the bureau was instructed to appoint a policy committee to examine and report upon the principles which should govern the admission of new unions. Some anxiety was shown regarding a resolution adopted by the Executive Board of Unesco recommending that grants-in-aid should be made principally to new projects, but it was decided at the General Conference of Unesco, which followed almost immediately after the General Assembly of ICSU, to continue the grants-in-aid which had given most useful and valuable help to Unesco and its unions.

The following were elected as the bureau for the next three years: president, A. von Muralt, Switzerland; vice presidents, E. Borel, France, and H. Solberg, Norway; members, D. J. N. Mukherjee, India, and E. Herbays, Belgium; immediate past president, John A. Fleming, U. S.; general secretary, F. J. M. Stratton, Great Britain.

F. J. M. STRATTON

The National Malaria Society will meet conjointly with the **American Society of Tropical Medicine**

and the **American Academy of Tropical Medicine** at the Hotel Peabody, Memphis, Tennessee, November 6–9. The National Malaria Society has scheduled three scientific sessions, including a joint meeting with the American Society of Tropical Medicine.

A symposium on dynamic properties of polymeric materials, sponsored by the Institute of Polymer Research, will be held November 19 at the Polytechnic Institute of Brooklyn. W. J. Hamburger, H. M. Morgan, and M. M. Platt, of Fabric Research Laboratories, Boston, will speak on "Mechanics of Ballistic Performance of Textile Materials"; A. V. Tobolsky, of Princeton University, on "Relations Between Dynamic and Static Properties of Polymers"; G. M. Kavanaugh and A. G. H. Dietz, of Massachusetts Institute of Technology, on "Ultrasonic Determination of Mechanical Properties of Synthetic Resin Adhesives"; and T. W. George, of the Naval Research Laboratory, Washington, D. C., on "Dynamic Localization of Plastic Flow."

New York University, in cooperation with the Atomic Energy Commission, will hold a three-day **conference on industrial and safety problems of nuclear technology,** January 10–12. Speakers include Gordon Dean, AEC commissioner, W. E. Kelley, manager of the New York Operations Office of AEC, G. K. Green, chief of the Accelerator Project at Brookhaven National Laboratory, Serge A. Korff, professor of physics at New York University, and G. Failla, professor of radiology, Columbia University.

The 18th International Physiological Congress will be held at Copenhagen August 15–18, 1950. Preliminary programs and registration forms will be sent out at the end of this year and will be obtainable, after January 1, from local societies of physiology, biochemistry, and pharmacology, or from the bureau of the congress, Zoofysiologisk Laboratorium, 32 Juliane Mariesvej, Copenhagen O.

The annual meeting of the **Scandinavian Pharmacological Society** will take place in Copenhagen, August 19–20, 1950. Members of pharmacological societies outside Scandinavia will be invited to attend this meeting.

Deaths

Charles F. Briscoe, 80, formerly head of the Department of Bacteriology at Mississippi State College, died on October 9. Dr. Briscoe was well known for his research on tuberculosis of farm animals and the response of soybeans to artificial inoculation.

Carl E. Seashore, 83, psychologist and dean emeritus of the Graduate School, University of Iowa, died October 16 in Lewiston, Idaho. Dr. Seashore, a native of Sweden, devised intelligence tests that were used by the U. S. Army during the first and second world wars. He was also known for his experimental work in the analysis and development of musical sound.

Raymond B. Wilcox, 60, pathologist at the U. S. Department of Agriculture station at Pemberton, New Jersey, died October 22 of a heart attack. Dr. Wilcox did research in diseases of small fruits and virus diseases of plants.

A National Foundation for Eye Research was recently formed in Washington, D. C., to encourage public support for research aimed at discovering the causes of ocular diseases leading to blindness, and developing more efficacious remedies. The activities of the foundation will be limited entirely to supporting and establishing research centers for ophthalmology, leaving other aid to the blind to existing agencies. George Swartz has been selected as national chairman. Allocation of research funds will be made upon the advice of a scientific advisory board consisting of V. Everett Kinsey, chairman, Francis Heed Adler, Edwin B. Dunphy, Jonas Friedenwald, and Alan C. Woods. Several more members will be added to the board as the need for expansion arises.

The Registry of Rare Chemicals, 35 West 33rd Street, Chicago 16, has submitted the following list of wanted chemicals: usneol, divaricatic acid, atranorin, shikimic acid, obtusatic acid, physodic acid, 1,2,4-trimethyl-3-hydroxyphenazine, berberine, 2,6-dinitro-4-hydroxylaminotoluene, senecioic acid, 2-4-diamino-6-methylpyrimidine, 2,3-dimercaptopropyl ethyl ether, butyl fluoride, azomethane, decafluorocyclopentane, carnegine, 2,3-dimercaptobutane, isobutyl peroxide, 4-amino-8-nitroquinoline, 2-methyl-8-hydroxyquinoline, apigenin, apiin, and canadine.

Bibliography of Books and Published Reports on Gas Turbines, Jet Propulsion, and Rocket Power Plants, Circular 482, by Ernest F. Fiock, has been published by the National Bureau of Standards and is now available from the Superintendent of Documents, U. S. Government Printing Office, Washington 25, D. C. at 20 cents per copy.

Make Plans for—

Gulf and Caribbean Fisheries Institute, second annual meeting, November 15–18, Robert Richter Hotel, Miami Beach, Florida.

American Institute of Chemical Engineers, New York Section, November 17, Hotel Statler, New York City.

American Society for Pharmacology and Experimental Therapeutics, first interim meeting, November 17–19, Indiana University Medical Center, Indianapolis.

American Physical Society, 295th meeting, November 25–26, University of Chicago.

Central Association of Science and Mathematics Teachers, November 25–26, Edgewater Beach Hotel, Chicago.

National Council of Geography Teachers, 35th annual meeting, November 25–26, Hotel Cleveland, Cleveland.

American Mathematical Society, November 25–26, Illinois Institute of Technology, Chicago; November 26, California Institute of Technology, Pasadena.